

A
D E F E N C E
OF THE
D O C T R I N E

Touching the

Spring and Weight

Of the A I R,

Propos'd by Mr. R. BOYLE in his New Phy-
sico-Mechanical Experiments;

Against the OBJECTIONS of

FRANCISCUS LINUS.

Wherewith the Objector's

FUNICULAR HYPOTHESIS

is also Exam'n'd.

By the Author of those Experiments.

L O N D O N,

Printed by M. Fleſher, for Richard Davis Bookſeller in
Oxford, 1682.

THE NEW YORK

LIBRARY

OF THE

ALBANY

AND

THE

NEW YORK

LIBRARY

The Publisher

TO THE

R E A D E R.

Friendly Reader,

YOU may possibly in this Volume have expected the Appendix which the Author heretofore promised, and has intended shall contain some additional Experiments to those which were formerly publish'd, and are here now reprinted in this Second Edition. These following Answers to Franciscus Linus and Mr. Hobbs are presented in compensation of the delay, and for your forbearance of that Appendix, which ere long you may expect in kind. For the Author having hinted the Promise seems thereby to acknowledge the Debt, and to be content to continue the Obligation to see it performed. And these ought the rather to be his excuse, because the writing these Answers, and publishing the Sceptical Chymist, and some other Discourses, have been the principal hindrances to that Piece; which is really so near a readiness, that part of it has lain at the Press these six Months. But yet it being not all perfected, the Stationer was loth to delay any longer the Publication of

The Publisher to the Reader.

of these, for which he has been so frequently call'd upon. And they (though a Latine Edition is intended) appear now the rather in English, that they may accompany the Second Edition of the Original Experiments, which were printed first in that Language in Octavo; and that instead of the promised Appendix they may complete the bulk of the Quarto Volume.

As for that part of this Piece that concerns Mr. Hobbs, it might have been larger: but the information that the Author had that the Learned Dr. Wallis was writing against some passages in Mr. Hobbs his Dialogues (as well that concerning the Air as the rest) was the occasion why his H. would make no Animadversion on some passages therein; and thought it not fit to enlarge upon others. And for the Errata of the Press I hope they will not be many: However the Author as to these is to be excused, who never (in regard to his Eyes and Impediments on other occasions) gives himself the trouble of Corrections and Revises; neither could the Publisher much attend the Press, it being printed in a distant place from his usual Abode. If, as I wish, you shall find this jealonsie of mine to have been causeless, you will have reason to give the Piece that is so kindly offered, and leads you such rare and untrodden paths in the best way of Natural Philosophy, the fairer entertainment and acceptance. Farewell.

RO. SH.

THE

THE AUTHOR'S
P R E F A C E
AND
DECLARATION.

THEY that know how indispos'd I naturally am to Contentiousness, will, I presume, wonder to see me publicly engaged in two Controversies at once. But that I am still as averse as ever from entering into Disputes that may handsomely be declin'd, the way wherein I have managed the following Controversies will, I hope, evince. And the Inducements I now have to appear in publick are such, that it would be hard for me to resist the being prevail'd on by them.

For, in the first place, I was (by Name, as it were) challenged by a person, who undertook to disprove not one or two of my Conjectures, but as much of the whole Body of my Treatise as concern'd the Spring of the Air, which most of my Explications suppose. And this being done by a Learned Man, who writes very confidently of the goodness of his *Hypothesis* and Arguments, and his Book being soon after follow'd by another written by Mr. *Hobbs*, a man of Name in the World; there seem'd to be some danger that so early an Opposition might oppress the Doctrine I had propos'd, before it was well understood and duly ponder'd. Wherefore I fear'd I might be wanting to the Truth and my self, if I should at such a time be altogether silent; especially since I might probably divert many who would otherwise be forward to appear against us, by letting them see how defensible our Doctrine is even against such Adversaries as those I have reply'd to. And this course I the rather chose, that in case I should henceforward comply with those who would have me forbear to write any further of these

The Preface.

Controversies, it might not be presently infer'd from my silence, that a good Cause cannot enable a Man no better than mine to defend it.

But I scarce doubt but that intelligent Readers, especially those that are imbued with the Principles of the Corpuscularian Philosophy, will be much more apt to think that I had reason to write the following Discourses, than to think that I had any to make them so prolix: And especially ingenious men, that are accustomed to admit nothing that either is not intelligible, or is precarious, will think divers of the Objections I reply to have needed no Answers, or at least no solemn ones. But to these I have four things to represent.

And First, That which not a little swells the bulk of the following Treatises, is the inserting those passages of my Adversaries that I examine in their own words: which being a Practice that I expect from any that shall think fit to animadvert upon any Opinion or Argument of mine; I thought it but equitable to do what I desir'd to have done to me, though oftentimes I could not do it in a little room.

Next, I was the more willing to prosecute some of *Franciscus Linus* his Objections, because the fear of being reduc'd to grant a *Vacuum* has so prevail'd with many eminent persons bred up in the received Philosophy of the Schools, that though they disagree both with him and among themselves about the particular manner of solving the *Phænomena* of the *Torricellian* Experiment; yet they agree in ascribing them to some extremely-rarefied substance that fills up the space deserted by the Quick-silver. So that this Opinion, as to the main, being approved by many eminent Scholars, especially of that most learned Order of the Jesuites, (to whom perhaps its Congruity to some Articles of their Religion chiefly recommends it) I was willing to pay them that respect, as not to dissent from persons, divers of whom for their eminence in Mathematicks and other Learning I much esteem, without shewing that I do it not but upon Considerations that I think weighty.

Thirdly, though the Examiners *Hypothesis* have but few,
and

The Preface.

and not very considerable, Arguments to countenance it; yet his Objections against our Doctrine (the Reply to which takes up the first Part of the following Treatise) are such, as though they may be solidly answered by any that thoroughly understands our *Hypothesis*, yet they may chance puzzle such Readers as do not, and these possibly will prove more than a few.

And, Lastly, because that sometimes when the Argument objected did not perhaps deserve to be much insisted on, the Argument treated of deserv'd to be considered; I thought it not amiss to make use now and then of some such opportunities to illustrate the matter it self under consideration: Which I the rather did for these two Reasons;

First, because I find that, except by some able Mathematicians and very few other contemplative men, the Doctrine of the Spring of the Air, at least as I have propos'd it, is not yet sufficiently apprehended, (and therefore needs to be inculcated.) Insomuch that through a great part of some late Discourses of men otherwise eminently learned, (written against other Elaterists, not me) there seems to run so great and clear a mistake, perhaps for want of skill in the *Hydrostaticks*, that I can scarce impute it to any thing, but to their not thoroughly understanding the *Hypothesis* they would confute.

And, Next, because I was willing to lay down in my Answer to the Objections I examin'd, the grounds of answering such other Arguments as may be built upon the same or the like Principles. And perhaps I may truly enough say, that in the following Treatise I have already in effect answered several discourses, written some before and some since mine, by learned men, about the *Torricellian* and other new Experiments relating to a *Vacuum*, though I forbore to mention the names or words of the Authors, because I found not that my Writings or Experiments were as yet known to them. To these things I may adde, that I thought the Discourses of *Linus* the fitter to be insisted on, because he seems to have more diligently than some others, (who yet venture to dispute against it) enquired into our Doctrine. And I shall not scruple to say thus much of an Adversa-

The Preface.

ry, (and one to whom I gave no provocation to be so) that though I dare not speak in general of those that have written either about the Weight of the Air, or else For or Against a *Vacuum*, because (as I acknowledge in the first Chapter following) I cannot yet procure the Books of divers learned men, especially of those great Personages, *Robertuall*, *Balianus* and *Casatus*; yet among the Writers I have hitherto met with, who have recourse to the *Aristotelean* Rarefaction and Condensation in the Controversies under debate, scarce any seems to have contrived his *Hypothesis* better than our *Linus*. Not that I think his Principle is either true, or (at least to such as I) intelligible; but that the *Funiculus* he assumes being allow'd him, he may, for a Reason to be touch'd a little below, make out, though not all the *Phenomena* of my Experiments, yet many more of them than most other Plenists, that deny the Spring of the Air, can deduce from their *Hypotheses* if granted. And in regard that, whereas we ascribe to the Air a Motion of Restitution outwards, he attributes to it the like Motion inwards, it cannot but happen that, though the Principles cannot both be true yet many of the *Phenomena* may be explicable by which of them soever is granted: because of this, I say, it is not so easie as many ingenious Readers may be apt to think, to draw pertinent Objections from Experience against the Adversary I have to deal with. Which I represent, lest, as some may think I have employ'd more Arguments than I needed, so others should think I have omitted many; as indeed I have omitted some, that I might pertinently have employ'd.

But there is another sort of Persons besides those I mention'd at the beginning of this Preface, to whom I must address the remaining part of it; namely, to those who seem troubled, that I suffer my self to be diverted either by *Linus* or Mr. *Hobbs* from perfecting those Experimental Treatises that are lying by me, almost promis'd by the learned Publisher of the Latine Edition of my *Essays*; and from prosecuting those wayes of enquiry into the Nature of things,

About the History
of Flame, of Heat,
of Colours, of the
Origine of Quali-
ties and Forms,
&c.

wherein

The Preface.

wherein they are pleas'd to think I may be more serviceable to real Learning and the Lovers of it. And I confess that these Mens Reasons and Perswasions have so far prevail'd with me, that after what I have done in the two following Treatises, to Vindicate my Writings from the Objections made against them by two Learned men of very differing *Hypotheses*, and thereby to shew in some measure that I am not altogether unacquainted with the way of defending oppos'd Truths, I have laid aside the thoughts of writing any more distinct or entire *Polemical* Treatises about the Subjects already disputed of. And to this I am invited by several other Reasons (besides what I have newly intimated.)

For first, as I elsewhere declare, it was not my chief Design to establish Theories and Principles, but to devise Experiments, and to enrich the History of Nature with Observations faithfully made and deliver'd; that by these, and the like Contributions, made by others, men may in time be furnish'd with a sufficient stock of Experiments to ground *Hypotheses* and *Theories* on. And though in my *Physico-Mechanical* Epistle and my *Specimens* I have ventur'd some Conjectures also at the Causes of the *Phænomena* I relate, lest the Discourse should appear to inquisitive Readers too jejune; yet (as I formerly said) I propos'd my Thoughts but as Conjectures design'd (though not onely, yet chiefly) to excite the Curiosity of the Ingenious, and afford some hints and assistance to the Disquisitions of the Speculative. And accordingly I have not forborn to mention divers things, which judicious Readers may easily perceive I foresaw that many would think unfavourable to the Opinions I inclin'd to. So that for me to leave Experimental for Controversial Studies, were a course unsuitable to the principal scope of my Writings.

Next, though I have adventur'd to improve the Doctrine of the *Spring* and *Weight* of the *Air* by some Supplements where I found it deficient, and to recommend it by some new Illustrations and Arguments deduc'd from my Experiments: yet the *Hypotheses* themselves (for the main) being the Opinions also of

The Preface.

far less able Men than I, it might be thought injurious both to them and to our common Cause; if I should needlessly go about to hinder them from the Honour of Vindicating the Truth; we agree in; especially, some of them being Excellent *Mathematicians*, and others Eminent *Naturalists*, whose Concern to maintain the *Hypotheses* against Objections, if any shall arise, is equal to mine, and whose leisure and abilities far exceed those of a Person who both is sickly, and hath other employments enough; and who (if he were far better skil'd in *Geometry* than he pretends to be) hath such a weakriess in his Eyes, as makes him both unwilling and unfit to engage in any Study where the conversing with Mathematical Schemes is necessary.

Thirdly, nor do I see much cause to doubt that the things I have deliver'd will notwithstanding my silence be left undefended: The forwardness I have already observ'd in divers *Virtuosi* to Vindicate those Writings, which they are pleas'd to say have convinc'd them, and to save me the labour of penning the following Treatises, scarce permitting me such an Apprehension. Especially, since there are some things that will much facilitate their Task, if not keep men from putting them upon it. For though Mr. *Hobbs* and *Linus* have examin'd my Writings upon Principles wherein they differ as much from each other as from me; yet neither have they seen cause to deny any thing that I deliver as Experiment, nor have their Objections been considerable, whether as to Number or to Weight, against the Applications I have made of my Principles to solve the *Phænomena*. So that usually without objecting any Incongruity to my particular Explications, they are fain to fall upon the *Hypotheses* themselves: in whose Defence I think I may with the more Reason expect to be seconded; because not onely I have endeavour'd, as I formerly noted, to lay the grounds of answering such Objections as I foresaw might arise; but I have also, to prevent or ease their labour, written the two first Parts of my Defence against *Linus*, without being oblig'd to do so for the Vindicating of my Explications, which are particularly maintain'd in the third Part.

I know not whether I may venture to adde on this occasion,

That

The Preface.

That those who have taken notice of the usefulness of Experiments to true Philosophy, and have observ'd that nevertheless the Difficulty, Trouble, and Charge of making them is such, that even in this Learned Age of ours there are very few *Bacon's* or *Mersennus's* to be met with, and those who have either made themselves, or at least seen others make Experiments, even such as those I have publish'd, with the care I am wont to think my self oblig'd to employ on such Occasions; will perhaps not only believe that they cost me far more time and pains than they that have not made nor seen such tryals are apt to imagine, but will possibly think it enough for a Person that is not by Profession a Scholar, to make them carefully, and set them down faithfully, and will allow him to let others Vindicate the Truths he may have the good fortune to discover, especially, when there are so many fitter for it than he, who have (as well as his Adversaries) more leisure to write Disputations than opportunity to prosecute Experiments; the latter of which to be perform'd as it ought to be, doth in many cases, besides some Dexterity scarce to be gain'd but by practice, require sometimes more Diligence, and oftentimes too more Cost, than most are willing or than many are able, to bestow upon them.

To be short, though if any thing very worthy to be taken notice of by me be suggested against any of my chief opinions or Explications, I may either take an occasion to say somewhat to it elsewhere, or at least have an opportunity to consider it in a Review, wherein I may alter, mend, supply, vindicate or retract divers Passages of my other Writings: yet I would not have it expected that I should exchange a Book with every one that is at leisure to write one against a *Vacuum*, or about the *Air*. Which Declaration I make, not that I think it will or ought to hinder any man from making use of his liberty to express a dissent, if he sees cause; but for these two Reasons.

The one, That my silence might not injure either the Truth or my self, by tempting men to think, that whatever I do not answer, I cannot; but might give unbiass'd and judicious Readers a Caution to allow as little of Advantage to the Writings of my adver.

The Preface.

adversaries upon the account of their being unanswer'd by me, as if I were no longer in the World. And the other, That I may not hinder those who would reply to such Adversaries by leaving them an apprehension that either I may prevent them, or they me. To conclude, I see no cause to despair, that whether or no my Writings be protected, the Truths they hold forth will in time in spite of opposition establish themselves in the Minds of men, as the Circulation of the Blood, and other formerly much contested Truths have already done. My Humour has naturally made me too careful not to offend those I dissent from, to make it necessary for any man to be my Adversary upon the account of Personal Injuries or Provocations. And as for any whom either Judgment or Envy may invite to contend, that the things I have communicated to the World deserved not so much Applause as they have had the luck to be entertain'd with; that shall make no Quarrel betwixt us: For perhaps I am my self as much of that mind as he; and however I shall not scruple to profess my self one of those who is more desirous to spend his time usefully, than to have the Glory of leaving nothing that was ever written against him unanswer'd; and who is more solicitous to pursue the wayes of discovering Truth, than to have it thought that he never was so much subject to Humane Frailties as to miss it.

DEFENCE

OF *MR. R. BOYLE'S* EXPLICATIONS OF
his *Physico-Mechanical* EXPERIMENTS, against

FRANCISCUS LINUS.

The I. Part.

*Wherein the Adversaries Objections against the Elaterists
are examined.*

CHAP. I.

A Newly-published Treatise, (*De Corporum inseparabilitate*), being brought to my Hands, I find several Chapters of it employ'd to oppose the Explications I ventur'd to give of some of my *new Experiments touching the Spring of the Air*. Wherefore though I am very little delighted to be engag'd in Controversies, and though I be not at present without Employments enough (of a private, and of a publick Nature) to make it unseasonable for me, to be by a Work of this sort diverted from them; yet for the Reasons specified in the Preface, I hold it not amiss to examine briefly what is objected against the thing I have delivered: and the rather, partly, because the learned Author, whoever he be (for 'tis the Title-Page of his Book that first acquainted me with the name of *Franciscus Linus*) having forborn provoking Language in his Objections, allowes me in answering them to comply with my

Inclinations and Custom of exercising Civility, even where I most dissent in point of Judgment. Besides, the Author himself has somewhat facilitated my Reply to him, by directing me in the ninth Page to some Books and Passages that I had not, when I published my Epistle, either seen or taken notice of. As indeed there are besides some of these several other Discourses that treat of the *Torrecellian* Experiments, which though by the names of their Authors I guess to be learnedly written, I have not to this day had opportunity to peruse, my stay in the remoter part of *Ireland* (whither Philosophical Books were not, in that time of publick Confusion, brought) having kept me from hearing of divers of them, till they were all bought up. Which I here mention, to excuse my self if I have not taken notice of some things or passages to be met within these Writings, which their Learned Authors or Inquisitive Readers might justly perhaps expect I should take some notice of, in case those Writings had fallen into my hands. But to digress no further.

'Tis true indeed, and it somewhat troubles me that it is so, that I can scarce promise my self to make my Adversary a Profelyte, since he without scruple assumes those very things as Principles, that to me seem almost as great Inconveniences as I would desire to shew any Opinion I dislike, to be liable unto. But since whatever Operation the following Discourse may have upon the Person that occasion'd it, I hope it may bring some satisfaction to those Philosophers who can as little as I understand the *Aristotelean* Rarefaction, and who will as well as I be backward to admit what they cannot understand; it shall suffice me to defend the Truths I have deliver'd, if I cannot be so happy as to convince my acute Adversary of them; and I shall not believe my labour lost, if this Discourse can contribute to the Establishment of some Notions in Philosophy that I think not inconsiderable in the minds of those whose clear Principles make me the most respect their Judgements, and for whose sakes I principally write.

Now though I be not in strictness oblig'd to defend any more than such of my own Explications as the Examiner has thought

fit to question, and those Particulars which I have added by way of Improvement to the two *Hypotheses* of the Spring and Weight of the Air; yet that I may the more effectually prosecute what I lately intimated I aim at in this Writing, and may as well illustrate my Doctrine as defend it, I shall divide the ensuing Treatise into three Parts; whereof the first is design'd to answer my Adversaries Objections against our Principles; the second shall examine the Funicular *Hypothesis* he would substitute in their stead; and the third shall contain particular Replies to what he alledges against some of my particular Explications.

CHAP. II.

ALthough our Author confesses in his second Chapter, that the Air has a Spring as well as a Weight, yet he resolutely denies that Spring to be near great enough to perform those things which his Adversaries (whom for brevities sake we will venture to call *Elasticks*) ascribe to it. And his whole fourth Chapter, as the Title declares, is employ'd to prove that the Spring of the Air is unable in a close place to keep the Mercury suspended in the *Torrecellian* Experiment. The proof of this Assertion he says is easie: But alledges two or three Arguments for it, which I think will be more easily answer'd than his Assertion evinc'd.

In the First he says that those Experiments concerning the Adhesion of ones finger, &c. which he had mentioned in the foregoing Chapter, *eodem modo se habent in loco clauso ac in aperto*. But the answering of this we shall suspend till anon; partly, because it may then be more conveniently examin'd, and partly, because our Author seems not to build much upon it, his chief Argument being that which he proposes in these words, *Cum tota vis hujus Elastici pendeat à refutato jam aeris equipondio cum digitis 29½ argenti vivi, ita ut nec plus, nec minus faciat hoc elastrium in loco occluso, quam sit per illud equipondium in loco aperto; manifestum est, cum jam ostensum sit fictitium plane esse hujusmodi equipondium, fictitium quoque esse tale elastrium.* Wherefore since all the validity of his Objection

Page. 20.

against the Spring of the Air depends upon his former Chapter, wherein he thinks he has disprov'd the Weight of the Air; it will behove us to look back into the former Chapter, and examine the four Arguments which he there proposes. But I must crave leave to vary from his method, and consider the third in the first place, because the removal of that Objection will facilitate and shorten the answer to the rest. His Third Argument therefore is thus set down.

Nam si Tubus viginti tantum digitorum (quo uti sumus in primo Argumento) non totus impleatur argento, ut prius, sed spatium aliquod inter digitum superiorem & argentum relinquatur in quo sit solus aer; videbimus subtracto inferiore digito superiorem non solum deorsum trahi, ut prius, sed etiam argentum jam descendere, idque notabiliter quantum viderimus extendi potest exigua illa aeris particula a tali pondere descendente. Unde si loco illius aeris ponatur aqua, aliusve liquor qui non tam facile extenditur, descensus nullus erit.

Hinc, inquam, contra hanc sententiam formatur argumentum: nam si extensus ille aer nequeat vel hoc viginti digitos argenti a lapsu sustinere, uti jam vidimus, quomodo quæso sustentabit 29? Certè hæc nullatenus reconciliari possunt.

But to this Argument, which he thinks so irreconcilable with his Adversaries Hypothesis he has himself furnisht them with an Answer in these words; *Dices forte ideo argentum in hoc casu descendere, quia deorsum trahitur ab aëre illo sese per suum Elaterium dilatante.* Which Answer I think sufficient for the Objection, notwithstanding the two exceptions he takes at it.

For first, whereas he saies, that sic deberet digitus potius a tubo repelli, quam eidem affigi, cui non minus sursum quam deorsum fiat huiusmodi dilatatio. He considers not, that though the endeavour of the included Air to expand it self be at first every way alike, yet the expansion it self in our case must necessarily be made downward, and not upward; because the Finger that stops the Tube being expos'd on the upper parts and the sides to the external Air, has the whole Weight and pressure of the Atmosphere upon it; and consequently cannot be thrust away but

but by a force capable to surmount that pressure: whereas on the lower side of the Included Air there is the Weight of the whole *Mercurial Cylinder* to assist the Spring of the Air, to surmount the Weight of the *Atmosphere* that gravitates upon the restagnant *Mercury*. So that the Air included and endeavouring to expand it self, finding no assistance to expand it self upward, and a considerable one to expand it self downward, it is very natural that it should expand it self that way whence it finds less resistance. As accordingly it will happen, till the Spring of the Air be so far debilitated by its Expansion, that its pressure, together with the weight of the *Mercury* that remains suspended, will but counter-balance, not overcome, the pressure of the outward Air upon the restagnant *Mercury*. And this explication may be confirm'd by this trial that I have purposely made, namely, that if instead of Quicksilver you employ Water, and leave as before in the Tube an Inch of Air, and then inverting it, open it under Water, you will perceive the included Inch of Air not to dilate it self any thing near (for I need not here define the Proportion) half so far as it did when the Tube was almost fill'd with *Mercury*; because the Weight of so short a *Cylinder* of Water does but equal that of between an Inch and an Inch and an-half onely of Quicksilver, and consequently the inward Air is far less assisted to dilate it self and surmount the pressure of the outward Air by the *Cylinder* of Water than by that of *Mercury*. And as for what our Author sayes, that if instead of Air, Water or some other Liqueur be left at the top of the Tube, the Quicksilver will not descend: the *Elatists* can readily solve that *Phenomenon*, by saying that Water has either no Spring at all, or but an exceeding weak one; and so scarce presses but by its Weight, which in so short a *Cylinder* is inconsiderable. Now the same solution we have given of our Examiners Objection, gives us also an account why the Finger is so strongly fastned to the upper part of the Orifice of the Tube it stops; for the included Air being so far dilated that an Inch, for example, left at first in the upper Part of the Tube, reaches twice or thrice as far as it did before the descent of the Quicksilver, its spring must be proportionably

portionably weakned. And consequently that part of the Finger that is within the Tube will have much less pressure against it from the dilated Air within, than the upper part of the same Finger will have from the unrarefied Air without. By which means the Pulp of the Finger will be thrust in (which our Author is pleas'd to call suckt in) as we shall ere long have occasion to declare in our Answer to his second Argument.

And having said thus much to our Authors first exception against the solution he foresaw we would give of his third Argument; we have not much to say at present to this second.

For whereas he sayes, *Concipi non posse quomodo aer ille*
 Pag. 17. *sic se dilatat, argentumque deorsum trusat, nisi occupando*
majorem locum: Quod tamen hi Authores quam maxime refugunt,
asserentes rarefactionem non aliter fieri, quam per corpuscula aut
vacuitates: I wish he had more clearly express'd himself, since as his words are couch'd I cannot easily guess what he means; and much less easily discern how they make an Argument against his Adversaries. For, sure he thinks them not so absurd; as to imagine that the Air can dilate it self, and thrust down the Mercury, without in some sense taking up more room than it did before: For the very word *Dilatation*, and the effect they ascribe to the included Air, clearly imply as much; so that I see not why he should say that they are so averse from granting the Air to take up more Place than before, especially since he takes notice in the former Chapter, that we compare the Expansion of the Air to that of compress'd Wooll; and since he here also annexes that we explicate Rarefaction either by Corpuscles or Vacuities. But this later Clause makes me suspect his meaning to be, that the *Elasticks* do not admit that the same Air may adequately fill more of Place at one time than at another; which I believe to be as true as that the self-same lock of compress'd Wooll has no more Hairs in it, nor does adequately fill more Place with them, when it is permitted to expand it self, than whilst it remain'd compress'd. But against this way of Rarefaction our Author here has not any Objection, unless it be intimated in these words, *Concipi non potest*: Which if it be,
 I shall

I shall need onely to mind him in this place, that whereas many of the chiefest Philosophers, both of Ancient and our own times, have profess'd they thought not the *Aristotelean* way of Rarefaction conceivable; and he acknowledges (as we shall see anon) that it is not clear; what the ablest of his Party (the modern *Plenists*) are wont to object against the way of Rarefaction he dislikes, is, that it is not true, not that it is not intelligible.

CHAP. III.

OUR Authors Second Objection (for so I reckon it) is thus propos'd by him. *Si sumatur tubus utrinque apertus sed longior, puta digitorum 40. Argentoque impleatur, eique digitus supernè applicatur ut prius, videbimus subtracto inferiore digito, argentum quidem descendere usque ad consuetam suam stationem; Digitum autem superiorem fortiter intra tubum trahi, eique firmissime, ut prius, adharere. Ex quo rursus evidenter concluditur, argentum, in sua statione constitutum, non ibidem sustentari ab externo aëre, sed à funiculo quodam interno suspendi, cujus superior extremitas, digito affixa, eum sic intra tubum trahit, eique affigit.* But this Argument being much of the same nature with that drawn from his third Experiment, the Answer made to that and to his first may be easily apply'd, and will be sufficient for this also; especially because in our present case there is less Pressure against the Pulp of the Finger in the inside of the Tube than in the third Experiment (where some Air is inلودed, though much expanded and weakned;) the Pressure of the Atmosphere being in the present case kept off from it by the subjacent Mercury, whereas there is nothing of that Pressure abated against the other parts of the Finger that kept it off from the deserted Cavity of the Tube, save onely that from the Pulp that is contiguous to the Tube, there may be somewhat of that Pressure taken off by the Weight of the Glass it self. But as for that Part of the Finger which immediately covers the hole, whether or no there be any Spring in its own fibres, or other constituent substances, which finding no resistance in the place deserted by the Quicksilver, may contribute to its swelling (for that we will not:

not now examine) he that has duly consider'd the account already given of this Intrusion of the Pulp into the Glass, will find no need of our Authors internal *Funiculus*, which to some seems more difficult to conceive, than any of the *Phænomena* in Controversie is to be explain'd without it.

CHAP. IV.

BY what we have already said against our Examiners Third Argument, we may be assisted to answer his first, though he propose it as a very clear Demonstration; and though it be indeed the principal thing in his Book. *Sumatur* (sayes he)

tubus brevior digitis 20; puta digitorum 20. non tamen clausus altero extremo, (ut hactenus) sed utrinque apertus: Hic Tubus, immerso ejus orificio Argento restagnanti, suppositoque digito, ne effluat Argentum Tubo infundendum, impleatur Argento vivo: aliusq; deinde digitus orificio quoq; applicetur, illudq; bene claudat. Quo facto, si subtrahatur inferior digitus, sentietur superior vehementer trahi ac sugi intra tubum, tamq; pertinaciter ei (vel argento potius, ut postea) adherere, ut ipsum tubum cum toto argento incluso facile elevet teneatq; in vase pendulum.

Ex quo sane experimento clarissimè refellitur hæc sententia: Cum enim, juxta eam, Argentum in tubo hujusmodi 20. tantum digitorum, sursum trudatur à præponderante aëre externo; nunquam profecto per eam explicabitur, quomodo digitus ille sic trahatur deorsum, tuboque tam vehementer adhareat; non enim à trudente sursum potest sic deorsum trahi.

Thus far our Authors objection, in answer whereunto I have divers things to represent, to shew, that a good account may be given of this Experiment in the *Hypothesis* of the *Elaterists*, which is sufficient to manifest how far the argument is from being so unanswerable as the proposer of it would persuade his Reader.

I deny then that the Finger is drawn downward, or made by suction to adhere to the Tube; but I explicate that which he calls the suction of the Finger, as I lately did in answer to his third Argument, as we shall more particularly see anon.

He sayes indeed, that the Air which thrust up the Quicksilver cannot

cannot so strongly draw down the Finger. As if the Air were not a fluid body, but a single and entire pillar of some solid matter. But to shorten our Reply to his Objections, the best way perhaps will be briefly to explicate the *Phænomenon* thus:

When the Tube is fill'd with Quicksilver, the Finger that stops the upper Orifice is almost equally press'd above and at the sides by the contiguous Air; but when the lower Finger is remov'd, then the Cylinder of *Mercury*, which before gravitated upon the Finger, comes to gravitate upon the restagnant *Mercury*, and by its intervention to press against the outward Air: so that against those parts of the Finger that are contiguous to the Air there is all the wonted pressure of the outward Air; whereas against that Pulp that is contiguous to the *Mercury* there is not so much pressure as against the other parts of the Finger by two thirds. I say by two thirds, or thereabout, because the *Mercurial* Cylinder in this Experiment is suppos'd to be twenty Inches high; and if it were but a little more than thirty Inches high, (which is a third more) then the weight of the Quicksilver would take off not two thirds only, but the whole pressure of the outward Air, from the above-mentioned pulp of the Finger. For in that case the Quicksilver would quite desert it, and settle beneath it. Wherefore since it has appeared by our Answer to the Examiner's third Argument, That the pressure of the outward Air is taken off from the body that remains in the upper part of the Tube, according to the weight of the Liquor suspended in the Tube; and since in our *Hypothesis* the pressure of the outward Air is able to keep thirty Inches of Quicksilver, or two or three and thirty foot of Water, suspended in a Tube; it need be no great wonder, if a pressure of the ambient Air, equal to the weight of a Cylinder of Water of near twenty two foot long, should be able to thrust in the pulp of the Finger at the upper Orifice of the Tube, and make it stick closely enough to the lip of it.

I know the Examiner affirms, That no thrusting or pressure from without can ever effect such an adhesion of the Finger to the Tube. But this should be as well prov'd as said. But, first,

though I am willing to think the Examiner would not knowingly relate any thing he is not persuaded of; yet as far as I and another person very well vers'd in these Experiments have purposely tried, I could not find the Adhesion of the Finger to the Tube to be near so strong as our Author hath related. Secondly, if you carefully endeavour by pressure and otherwise to thrust the pulp of your Finger into the Orifice of the Tube, you may through the Glass perceive it to be manifestly tumid in the cavity of the Pipe. And if by pressing your Finger against the Orifice of the Tube, you should not make the pulp adhere quite so strongly to the Tube, nor swell quite so much within it, as may happen in some *Mercurial* Experiments; it is to be consider'd, that the Air being a fluid as well as a heavy body, it does not (as grosser Weights would) press only against the upper part of the Finger, but pressing as much of the finger as is expos'd to it almost every where, and almost uniformly, as well as strongly, it does by its lateral pressure on every side thrust in the Pulp of the Finger into the hole where there is not any resistance at all, or at least near so much pressure against the Pulp as that of the ambient Air against the parts of the Finger contiguous to it.

By this it may appear that we need not borrow the Objection our Author offers to lend us; namely, that in the Experiment under consideration the Quicksilver is press'd downward by the Spring of some Air lurking betwixt it and the Finger. (Though I am prone to think that unless the Experiment be made with a great deal of care, such a thing may easily happen, and contribute to the stronger Adhesion of the Finger to the Tube.) This I say may appear notwithstanding what our Author Objects, that the Air expanding it self will thrust away the Finger upwards, since the contrary of that pretence we have lately manifested in the Answer to his Third Argument. And as for what he adds to confirm his Argumentation in these words, *Quod vel inde confirmatur, Quia cum pre-*

Page 14. *ponderans ille aer succedat (uti asseritur) loco sublato inferioris digiti, id est, eodem modo nunc sustentet Argentum quo ante*

ante ab applicato digito inferiore sustentabatur; manifestum est, non debere, juxta hanc sententiam, magis deorsum trahi digitum superiorem post sublatam inferiorem quam ante. Cum itaque contrarium plane doceat experientia, satis liquet sententiam illam esse falsam. We must consider that the Tube being suppos'd perfectly full of Mercury, the Finger that stops the lower Orifice is wont to be kept strongly press'd against it, lest any of that ponderous Liquor should get out between the Tube and the Finger. So that although both the lower Finger do indeed keep up the Mercury in the Tube, and the pressure of the outward Air would do so too; yet there is this difference, that the pressure of the Atmosphere depending upon its Weight, cannot be intended and weakned as we please, as can that of the undermost Finger. And therefore whereas the Atmospheric Cylinder will not keep up a Cylinder of Quicksilver of above thirty Inches high, those that make the Torricellian Experiment do often, upon one occasion or other, keep up with the Finger a Mercurial Cylinder of perhaps forty or fifty Inches or far more: So that whereas in our case, before the removal of the undermost Finger, the Pulp of the uppermost must have about the same pressure against it where it is contiguous to the Mercury, as there is against the other part of the same Finger; after the removal of the undermost Finger, there is as much of the Atmospheric Pressure, if I may so speak, taken off from the newly mention'd Pulp as counter-balances a Cylinder of Quicksilver of twenty Inches long.

CHAP. V.

THe Examiners Fourth and last Experiment is thus propos'd. Quarto denique (says he) impugnatur: Quia ex eo sequeretur, Argentum vivum per similem Tubum & vasculo exungi posse eodem prorsus facilitate quâ ex eodem exugetur aqua: quod tamen experientia repugnat, quâ docemur aquam in oro surgentem facillime attrahi, quo tamen Argentum vivum, ne tato quidem adhibito conatu perducitur, imo vix ad Tubi medietatem.

Page 18.

Sequelam autem sic ostendo: Quia cum in hac sententia nihil aliud agendum sit quam hoc, ut per Tubum sic ascendat subiectus Liquor, sive Aqua fuerit, sive Argentum, nisi ut sugendo sursum trahatur aer Tubo inclusus, quo sic attracto ascendit illico subiectus Liquor, protrusus nimirum ab externo aere jam preponderante (uti docet Pecquetus in dissertatione Anatomica pag. 63.) manifestum est, eadem planè facilitate exugendum sic Argentum vitrum qua exugitur Aqua: Quod quum Experientie tam aperte repugnat, necesse est sententiam ex qua sequitur falsam esse.

This Experiment I remember I made some years ago, accordingly 'tis allerdg'd in the fourth Essay of the Treatise (I was then writing) to prove against the Vulgar Opinion, that Liquors do not to prevent a *Vacuum* spontaneously ascend, which I presume will be so far allow'd of by our Author, who would have Liquors suppos'd to be rais'd by Suction violently drawn up by the contraction of his *Funiculus*. But to examine this Experiment, as it concerns the present Controversie, we may recal to mind that we formerly shew'd in the Answer to our Author's Third Argument, That when the *Mercurial Cylinder* that leans upon the restagnant *Mercury* has at the other end of it Air, kept from any entercourse with the *Atmosphere*, that included Air has so much of the Pressure of the external Air taken off from it as counterpoises the *Mercurial Cylinder*. And the Finger that is expos'd to the whole Pressure of the ambient Air in some of its Parts, and in others but to the much fainter Pressure of the included Air, endures an unufal Pressure from the preponderating power of the *Atmosphere*.

We may consider also that there is against the *Thorax* and those Muscles of the *Abdomen* that are subservient to Respiration the Pressure of the whole ambient Air. Which Pressure, notwithstanding, The Muscles design'd for the use of Respiration, are able without any considerable resistance to dilate the *Thorax* at pleasure; because, as fast as they open the Chest, and by dilating it weaken the Spring of that Air which is then within

within the Body, the external Air by flowing in, so want of finding the usual resistance there, keeps that within the *Thorax* in an *Æquilibrium* of force with that without. These things premised, 'tis not Difficult in our *Hypothesis* to give an Answer to our Examiner's Experiment. For we say when a *Cylinder* of *Mercury* is rais'd in the Tube to any considerable height, the Pressure of the Air in the *Thorax* is lessen'd by the whole weight of that *Mercurial Cylinder*, and consequently the Respiratory Muscles are thereby disabled to dilate the Chest as freely as they were wont, by reason of the prevalence of the undiminish'd Pressure of the external Air against the weakned Pressure of the internal: But if instead of *Mercury* you substitute Water, so short a *Cylinder* of that comparatively light Liquor takes off so little of the Pressure of the included Air, that it comes into the Lungs with almost its usual strength, and consequently with almost as much force as the outward Air presses with against the *Thorax*.

And on this occasion there occurs to my thoughts a noble Experiment of the most Ingenious Monsieur *Paschal*, which clearly shews, that if we could free the upper part of such a Tube as we are now considering from the Pressure of all internal Air, it would follow, as the Examiner says it should, that the Quicksilver would by the Pressure of the outward Air be impell'd up into the Tube as well as Water, till it had attain'd a height great enough to make its Weight not inferiour but equal to that of the *Atmosphere*. The Experiment it self being so pertinent and considerable, we shall annex it in the same words wherein it is related by his Country-man and Acquaintance, the Learned and Candid *Gassen-*
dw. Neque hoc verò solum, sed insuper vitreo
Diabete Clysteræ ea qua par fuerit longitudine
 confectò, & post embolum ad orificium usq; com-
 pulsus, immisso ad normam in subiectum Hydrargyrum, depre-
 bendit, ubi embolum sensim deinde educitur, consequi Hydrargy-
 rum ascendereq; ad eandem usq; duorum pedum & digitorum trium
 cum semisse altitudinem. To which he immediately subjoyns a

*Gass. Phys. Sect. 1.
 Lib. 2. Pag. 204.
 De nupero Inanis Ex-
 perimento.*

Circumstance very considerable to the present Controverſie in the following Clause. *Ac ubi deinceps, addibita licet non majore vi, Embolum altius educitur, conſiſtere Hydrargyrum, neq; amplius conſequi, ac fieri interim Inane quod ſpatium intercipitur ab ipſo ad Embolum uſque.* Thus far be. So that as to the Examiner's Experiment, we may well explicate it in our *Hypotheſis*, by ſaying, that agreeably to it it happens, that in a more forcible Reſpiration the *Mercurial Cylinder* is raiſed higher than in a more languid; becauſe, in the former Caſe, the Cheſt being more dilated, the included Air is alſo more expanded; whereby its debilitated Spring cannot as before enable the *Mercurial Cylinder* to counterpoize altogether the Preſſure of the ambient Air. And that the reaſon why the Quickſilver is not by Reſpiration raiſ'd as high as it is kept ſuſpended in the *Torricellian* Experiment, is not, that the Preſſure of the outward Air is unable to raiſe it ſo high, but becauſe, as we have already declar'd, the free Dilatation of the *Thorax* is oppoſed by the Preſſure of the ambient Air; which Preſſure being againſt ſo great a Superficies, and being but imperfectly reſiſted by the debilitated Preſſure of the Air within the *Thorax*, will be eaſily imagined to be very conſiderable by him who conſiders that in our Engine, the Preſſure of the external Air againſt the Sucker of leſs than three inches Diameter was, as we relate in the 33. Experiment; able to thruſt up a Weight of above a hundred pound. And here we may obſerve upon the By in confirmation of our former Doctrin, that when we ſtrongly ſuck up Quickſilver in a Glaſs Tube, though the Elevation of the Quickſilver be according to our Author performed likewise by his *Funiculus*, contraiſing it ſelf every way, and though there be a Communication between the internal ſurface of the Lungs, and the cavity of the Tube; yet we ſeel not in our Lungs any endeavour of the ſmoking *Funiculus* to tear off that Membrane they are ſind with.

And thus we have examin'd our Author's four Arguments, to prove that in the *Torricellian* Experiment the Quickſilver cannot be kept ſuſpended by the counterpoize of the external

Air:

Air: Against which Opinion he tells us Indeed, that other Arguments might be alledg'd, but as it is not probable that if he had thought them better than those he has elected to insist on, he would have omitted them; so 'tis not unlikely that Answers might be as well found for them as for the others; especially since that which he singles out for a *Specimen* is, that from his Adversaries Hypothesis it would follow, that the Quicksilver would descend much more (I suppose 'tis a mistake of the Press, for much less) in cold Weather than in hot, because the Air is then thicker and heavier, and therefore ought to impel up the Quicksilver higher. For besides that we shall in its due place question the validity of our Author's Consequence; it will be here sufficient to Reply, that the Observation on which he grounds it does not constantly hold, as his Objection supposes: Which may appear by that part of our 18. Experiment whence the matter of fact is desum'd, as we shall have occasion to take further notice of when we shall come to the Defence of that Experiment. So that what has been hitherto Discours'd on both sides being duly consider'd, the Reader is left to judge what ground the Examiner had for the *Empo'mpca* wherewith he is pleas'd to conclude his Third Chapter, *Maneat igitur tot Argumentis comprobatum, quorum quodlibet se solo sufficit, Argentum (facto Experimento in loco aperto) per externi aëris gravitatem à lapsu minime sustentari.* Pag. 19.

CHAP. VI.

His fourth Chapter, wherein the Title promises that he will prove, *Argentum in loco occluso non sustentari à lapsu per ipsum aërem Elaterium*, is very short, and does not require that we should dwell long upon it. For the proof he brings of his Assertion being this, *Cum tota vis hujus Elaterii pendat aresato jam aëris equipondio cum digitis 29½ Arge-
geant vix, ita ut ne plus nec minus faciat hoc elaterium in loco occluso quam sit per illud equipondium in loco aperto; manifestum est, cum jam ostensum sit fictitium plane esse hujusmodi equipondium,* Pag. 20.

dium, fixitium quoq; esse tale elaterium: This being no new Argument, but an Inference from those he had set down in the former Chapter, by our Answers to them it is become needless for us to make any distinct Reply to this. We shall rather desire the Reader to take notice, that whereas our Author says that according to his Adversaries, *Nec plus nec minus faciat hoc Elaterium in loco occluso quam sit per illud Aequipondium in loco aperto;* whatever others may have written, we for our part allow of this Opinion but in some Cases; for in others we have perform'd much more by the Spring of the Air, which we can within certain limits increase at pleasure, than can be perform'd by the bare weight, which for ought we know remains always somewhat near the same. And of this advantage that the spring of the Air may have in point of force above the weight of it, we have formerly given an Instance in our 17. Experiment, (where, by compressing the Air in the Receiver, we impell'd the *Mercurial Cylinder* higher than the station at which the counterpoise of the Air is wont to sustain it) and shall hereafter have occasion to give yet more considerable proofs. To the lately recited words our Examiner subjoyns these;

Adde, cum allata jam capite præcedente experimenta
 Pag. 21. *de adhesionē digiti, &c. eodem modo se habent in loco clauso ac in aperto, necessarium esse facta ex eis argumenta contra æquipondium, eadem quoq; contra elaterium vim habere.* But though he propose this as a new Argument, yet since 'tis built but upon the adhesion of the Finger (of which we have already given an account in our *Hypothesis*.) I see not how it requires any new and particular Answer. And whereas he says, that the Experiments he had mentioned concerning the adhesion of ones Finger, &c. *eodem modo se habent in loco clauso ac in aperto;* I could wish he had added what way he took to make the Trials. For he gives no intimation that he did them any other ways than in ordinary rooms. And in such there scarce ever wants a communication betwixt the inward and outward Air, either at the Chimney, or Window, or Door not exactly shut, or at some hole or crevice or other, by

by means of which the weight of the Atmosphere has its operation within the room.

To his second Argument our Author adds not a third, unless we take that for an Argument which he immediately annexes to his last recited words: *Et profecto* (says he) *si secum* Pag. 21.
exponderent bi Authores, quanta sit difficultas explicandi
hujusmodi aëris elaterium, nisi idem aër se solo occupet majorem
locum (ut paulo ante) credo eos sententiam facile mutaturos.

But this being said *gratis*, does not exact an Answer; and he must make it more intelligible than any man that I know of has yet done, how the same Air can adequately fill more space at one time than at another, before he perswade me to change my opinion about the Spring of the Air: Especially Pag. 11.
since he himself allows that the Air has a Spring, whereby it is able, when it has been violently compress'd, to recover its due extension; the manner whereof if he will intelligibly explicate, his Adversaries will have no great difficulty to make out the spring of the Air. But whether his *Hypothesis*, or ours, be the more intelligible, will be more properly considered in the second part of our Discourse, to which we will therefore now proceed.

The II. Part.

Wherein the Adversaries Funicular Hypothesis is
examined

CHAP. I.

What is alledged to prove the Funiculus is consider'd; and some
Difficulties are propos'd against the Hypothesis.

THE Hypothesis that the Examiner would, as a better, substitute in the place of ours, is, if I mistake it not, briefly this; That the things we ascribe to the weight or spring of the Air are really perform'd by neither, but by a certain *Funiculus*, or extremely thin substance, provided in such cases by Nature, *ne detur vacuum*, which being exceedingly rarefied by a forcible distension, does perpetually and strongly endeavour to contract it self into dimensions more agreeable to the nature of the distended body; and consequently does violently attract all the bodies whereunto it is contiguous, if they be not too heavy to be remov'd by it.

But this Hypothesis of our Authors does to me, I confess, appear liable to such Exceptions, that though I dislike'd that of his Adversaries yet I should not imbrace his, but rather wait till time and further Speculations or tryals should suggest some other Theory, fitter to be acquiesc'd in than this; which seems to be partly precarious, partly unintelligible, and partly insufficient, and besides needless: though it will not be so convenient to prove each of these apart, because divers of my Objecti-
onstend to prove the Doctrine, against which they are alledged, obnoxious to more than one of the imputed Imperfections.

First, then, the Arguments by which our Author endeavours to evince his *Funiculus*, are incompetent for that end.

The

The Arguments which he proposes in his sixth Chapter, (where he undertakes to make good his Assertion) I there find to be three.

The first he sets down in these words, *Constat hoc primò ex jam dictis Capite præcedente: nequit enim argentum descendens sic digitum deorsum trahere, tuboq; affigere, nisi à tali Funiculo suspendatur, eumq; suo pondere vehementer extendat, ut per se patet.* But to this proof answer has been made already in the former Part of this Discourse: onely whereas the Author seems to refer us to the foregoing Chapter, we will look back to it, and take notice of what I find there against the Vacuists. For though I neither am bound, nor intend, in this Discourse to declare my self for, or against a *Vacuum*; yet since I am now writing against the *Funicular Hypothesis*, it will much conduce to shew that it is not firmly grounded, if I examine what he here alledge; against the Assertors of a *Vacuum*. Page 24.

In the next place therefore I consider that according to the Examiner, there can be no *Vacuum*; and that he makes to be the main reason why Nature in the *Torricellian* and our Experiments does act after so extraordinary a manner, as is requisite to the production of his *Funiculus*. For in the 47th. Page, having in his Adversaries name demanded what need there is at the descent of the Quicksilver, that before it falls a *superficies* should be separated from it, and extended; *Respondeo* (says he) *ideo hoc fieri, ne detur vacuum; cum nihil aliud ibi adsit quod loco argenti descendentis possit succedere.* To which he immediately subjoyns, (with what cogency I will not now examine) *Atq; hinc plane confirmatur commune illud per tot jam elapsa secula usurpatum in Scholis axioma, viz. Naturam à vacuo abhorрere.* And though he seem to make his *Funiculus* the immediate cause of the *Phænomena* occurring in the *Torricellian* and our Experiments: yet that, if you pursue the inquiry a little higher, he resolves them into Nature's abhorrence of a *Vacuum*, himself plainly informs us in the next page; *Nam licet* (says he) *immixturatio extranea v. g. ex hydra bartholana superne clausa (quo exemplo utuntur) non descendat, non sit metus va. ni,* Page 48.

sed ea quam modo diximus, nempe quod non detur sufficiens pondus ad solvendum illum nexum quo adhaereat aqua clausæ hydræ summitatibus, ad eam tamen rationem tandem necessario veniendum est. But, though as well our Author's *Funiculus*, as the other scarce conceivable *Hypotheses* that learned men have devised, to account for the suspension of the Quicksilver otherwise than by the resistance of the external Air, seem to have been exco-
 gitated onely to shun the necessity of admitting a *Vacuum*: yet I see not how our Examiner cogently proves, either that there can be none in *rerum naturâ*, or that *De facto* there is none produc'd in these Experiments. For in his fifth Chapter (where he professedly undertakes that task) he has but these two incompetent Arguments. The first is drawn from the attraction, as he supposes, of the Finger into the deserted cavity of the Tube in the Torricellian Experiment: *Que quidem* (sayes he) *tam vehemens tractio & adhesio, cum non* Page. 22.
nisi à reali aliquo corpore inter digitum & argentum constitutum queat provenire, manifestum est spatium illud vacuum non esse, sed verâ aliquâ substantiâ repletum. But to this Argument having already given an Answer, let us (without staying to urge, that the Vacuists will perhaps object, that they see not a Necessity, though they should admit of Traction in the case, that the internal substance must therefore perfectly replanish the deserted Cavity; without pressing this, I say, let us) consider his other, which he draws from the Diaphaneity of the deserted part of the Tube, which space (he sayes) were it empty, would appear like a little black Pillar, *Eo quod nulla species visuales neq; ab eo neq; per illud possunt ad oculum pervenire.* But (not to engage our selves in Optical Speculations and Controversies) if we grant him somewhat more than perhaps he can prove; yet as the Experiment will not demonstrate that there is nothing of body in any part of the space deserted by the Mercury, so neither will the Argument conclude (as the Proposer of it does twice in this Chapter) That space verâ aliquâ substantiâ repleri. For according to the Hypothesis of the Epicureans and other Atomists, who make Light to be a corporeal Effluvi-
 from

from lucid bodies, and to consist of Atoms so minute, as freely to get in at the narrow Pores of Glas, there will be no cause to deny interspers'd Vacuities in the upper part of the Tube. For the Corpuscles of Light that permeate that space may be so numerous, as to leave no sensible part of it un-lightned; and yet may have so many little empty Intervals betwixt them, that, if all that is corporeal in the space we speak of were united into one lump, it would not perhaps adequately fill the one half (not to say the tenth, or even the hundredth part) of the whole space: According to what we have noted in the 17. Experiment, that a Room may appear full of the smoke of a Perfume, though if all the Corpuscles that compose that smoke were re-united, they would again make up but a small Pastil. To which purpose I remember I have taken *Camphire*, of which a little will fill a Room with its odour, and having in well-clos'd distillatory Glasses caught the Fumes driven over by heat, I thereby reduc'd them to re-conjoyn into true *Camphire*, whose bulk is very inconsiderable in comparison of the space it fills as to sense, when the odorous Corpuscles are scattered through the free Air.

To which I might adde, that the *Torricellian* Experiment being made in a dark night, or in a Room perfectly darkn'd, if it succeed (as there is little cause to suspect it will not) it may well be doubted whether our Authors Argument will there take place. For if he endeavour to prove that the place in question was full in the dark, because upon the letting in of the Day, or the bringing in of a Candle, the light appears within it; the Vacuists may reply according to their *Hypothesis*, That that light is a new one, flowing from the lucid body that darts its corporeal beams quite through the Glas and Space we dispute about, which for want of such Corpuscles were not just before visible.

And supposing light not to be made by a trajection of Atoms through Diaphanous bodies, but a propagation of the impulse of lucid bodies through them; yet it will not thence necessarily follow, that the deserted part of the Tube must be full: As in our 27. Experiment (though many of those gross Aërial

Particles that appear'd necessary to convey a languid sound were drawn out of our Receiver at the first and second Exsuction; yet there remain'd so many of the like Corpuscles, that those that were wanting were not mis'd by the sense, though afterwards, when a far greater number was drawn out, they were) so there may be matter enough remaining to transmit the impulse of light; though betwixt the Particles of that matter there should be store of vacuities intercepted. Whereas our Author pretends to prove, not onely that there is no *conserve* Vacuity in the space so often mention'd, but absolutely that there is *none*. For 'tis in this last sense, as well as the other, that the Schools and our Author, who defends their Opinion, deny a *Vacuum*.

But notwithstanding what we have now discours'd, as in our 17. Experiment we declin'd determining whether there be a *Vacuum* or no; so now what we have said to the Examiners Argument, has not been to declare our whole sense of the Controversie, but onely to shew, that though his *Hypothesis* supposes there is no *Vacuum*, yet his Arguments do not sufficiently prove it: which may help to shew his Doctrine to be precarious; for otherwise the *Cartesians*, though Plenists, may plausibly enough (whether truly or no I now dispute not) decline the necessity of admitting a *Vacuum* in the deserted space of the Tube, by supposing it fill'd with their second and first Element, whose Particles they imagine to be minute enough freely to pass in and out through the Pores of Glas. But then they must allow the pressure of the outward Air to be the cause of the suspension of the Quicksilver: for though the *materia celestis* may readily fill the spaces the *Mercury* deserts; yet that within the Tube cannot hinder so ponderous a liquor from subsiding as low as the stagnant *Mercury*; since all the parts of the Tube, as well the lowermost as the uppermost, being pervious to that subtle matter, it may with like facility succeed in whatever part of the Tube shall be forsaken by the Quicksilver.

The Examiners second Argument in the same place is, That since the *Mercurial Cylinder* is not sustain'd by the outward Air, it must necessarily be, that it be kept suspended by his internal

internal string. But since for the proof of this he is content to refer us to the third Chapter; our having already examin'd that, allows us to proceed to his third Argument, which is, That the *Mercurial Cylinder*, resting in its wonted station, does not gravitate: as may appear by applying the Finger to the immersed or lower Orifice of the Tube. Whence he infers, that it must of necessity be suspended from within the Tube. And indeed if you dexterously apply your Finger to the open end of the Tube, when you have almost, but not quite, lifted it out of the restagnant *Mercury*, (which circumstance must not be neglected, though our Author have omitted it) that so you may shut up no more Quicksilver than the *Mercurial Cylinder* is wont to consist of, you will find the Experiment to succeed well enough: (Which makes me somewhat wonder to find it affirm'd, that the learned *Maignan* denies it) not but that you will feel upon your Finger a gravitation or pressure of the Glass-Tube, and the contained *Mercury* as of one body; but that you will not feel any sensible pressure of the *Mercury* apart; as if it endeavoured to thrust away your Finger from the Tube. But the reason of this is not hard to give in our *Hypothesis*; for according to that, the *Mercurial Cylinder* and the Air counterpoising one another, the Finger sustains not any sensibly-differing pressure from the ambient Air that presses against the Nail and sides of it, and from the included Quicksilver that presses against the Pulp. But if the *Mercurial Cylinder* should exceed the usual length, then the Finger would feel some pressure from that surplusage of Quicksilver, which the Air does not assist the Finger to sustain. So that this pleasant *Phenomenon* may be as well solv'd in our *Hypothesis*, as in the Examiners: in which if we had time to clear an Objection, which we fore-see might be made, but might be answer'd too, we would demand why, when the *Mercury* included in the Tube is but of a due altitude, it should run out upon the removal of the Finger that stops it beneath, in case it be sustain'd onely by the internal *Funiculus*, and do, according to his Doctrine, when the *Funiculus* sustains it, emulate a solid body,

if

if the pressure of the external Air has not (as our Author teaches it not to have) any thing to do in this matter.

And if some inquisitive person shall here object, That certainly the Finger must feel much pain by being squeez'd betwixt two such pressures, as that of a Pillar of thirty Inches of Quicksilver on the one side, and an equivalent pressure from the Atmospherical Pillar on the other, it may readily be represented, that in fluid bodies (such as are those concern'd in our Difficulty) a solid body has no such sense of pressure from the ambient bodies as (unless Experience had otherwise instructed us) we should perhaps imagine. For, not to mention that having inquired of a famous Diver, whether he found himself sensibly compressed by the Water at the bottom of the Sea; he agreed with the generality of Divers in the Negative: I am inform'd that the learned *Maignan* did purposely try, that his hand being thrust three or four Palmes deep into Quicksilver, his fingers were not sensible, either of any weight from the incumbent, or of any pressure from the ambient, Quicksilver. The reason of which (whether that inquisitive man have given it or no) is not necessary in our present Controversie to be lookt after.

To these three Arguments the Examiner addes not a fourth, unless he design to present it us in this concluding passage: *Huc*
 Page 25. *etiam faciunt insignes librationes quibus argentum subito descendens agitur: Idem enim hic fit quod in aliis Pendulis & ab alto demissis fieri solet.* But of this Phenomenon also 'tis easie to give an account in our Hypothesis by two several ways; whereof the First (which is proper chiefly when the Experiment is made in a close place, as our Receiver) is, That the Quicksilver by its suddendescent acquires an *impetus* super-added to the pressure it has upon the score of its wonted gravity; whereby it for a while falls below its station, and thereby compresses the Air that leans upon the restagnant Mercury. Which Air by its own Spring again forcibly dilating it self to recover its former extension, and (as is usual in Springs) hastily flying open, expands it self beyond it, and thereby impells up
 the

the Quicksilver somewhat above its wonted station, in its fall from whence it again acquires somewhat (though not so much as before) of *Impetus* or power, to force the Corpuscles of the Air to a Sub-ingression; and this reciprocation of pressure betwixt the Quicksilver and the outward Air decreasing by degrees, does at length wholly cease, when the *Mercury* has lost that super-added pressure, which it acquired by its falling from parts of the Tube higher than its due station. But this *first* way of Explicating these Vibrations is not necessary in the free Air: For if we consider the ambient Air onely as a weight, and remember what we have newly said of the *impetus* acquir'd by descent; this *Phænomenon* may be easily enough explain'd, by taking notice of what happens in a Balance, when one of the equiponderant Scales chancing to be depressed, they do not till after many Vibrations settle in *equilibrio*.

And on this occasion I shall adde this Experiment: I took a Glass Pipe, whose two legs (very unequal in length) were parallel enough, and both perpendicular to that part of the Pipe that connected them; (such a *Syphon* is describ'd in our 36. Experiment, to find the proportion of the gravity of *Mercury* and Water) into this Quicksilver was pour'd till 'twas some Inches high, and equally high in both legs: then the Pipe being inclin'd till the most part of the Quicksilver was fallen into one of the legs, I stop't the Orifice of the other leg with my Finger, and erecting again the Pipe, though the Quicksilver were forc'd to ascend a little in that stop't leg; yet by reason my Finger kept the Air from getting away, the Quicksilver was kept lower by a good deal in that stop't leg than in the other; but if by suddenly removing my Finger I gave passage to the included and somewhat compress'd Air, the preponderant Quicksilver in the other leg would with the *Mercury* in this unstopt leg, make divers undulations before that liquor did in both legs come to rest in an *equilibrium*. Of which the Reason may be easily deduc'd from what has been newly deliver'd; and yet in this case there is no pretence to be made of a *Funiculus* of violently distended Air to effect the Vibrations of the *Mercury*.

CHAP. II

Divers other Difficulties are objected against the Funicular Hypothesis

THirdly, But though our Examiner have not sufficiently proved his *Hypothesis*, yet perhaps it may be in its own nature so like to be true, as to deserve to be imbrac'd as such. Wherefore we will now take notice of some of those many things that to our apprehension render it very improbable.

And first, whereas our Author acknowledges that Quicksilver, Water, Wine, and other Liquors, will, as well one as another, descend in Tubes exactly sealed at the top, in case the Cylinder of liquor exceed the weight of a Mercurial Cylinder of 29½. Inches; and will subside no longer than till it is come to equiponderate a Cylinder of Quicksilver of that height; whereas, I say, the Examiner is by the ingenious Monsieur Paschall's, and other Experiments, induc'd to admit this; it cannot but seem strange that, whatever the liquor be, there should be just the same weight or strength to extend them into a *Funiculus*: though Water, for instance, and Quicksilver be near fourteen times as heavy one as the other, and be otherwise of very distant natures; and though divers other liquors, as Oyle and Water, be likewise of Textures very differing. And this may somewhat the more be wondred at, because our Author (in his Animadversions upon our 31. Experiment) is pleas'd to make so great a difference betwixt the disposition of bodies of various consistencies, as fluid and firm, to be extended into a *Funiculus*, that he will not allow any humane force to be able to produce one, by the division of two flat Marbles, in case the contact of their Surfaces were so exquisite as quite to exclude all Air; though in the same place his Ratiocination plainly enough teaches (which Experience however does) that adhering Marbles, though with extraordinary difficulty, may be forcibly sever'd, and according to him the superficial parts may be distended into a *Funiculus*, that prevents a *Vacuum*.

But now the *Hypothesis* of his Adversaries is not at all incum-

cumbred with this difficulty. For the weight of the outward Air being that which keeps liquors suspended in Tubes sealed at the top; it matters not of what nature or texture the suspended liquor is, provided its weight be the same with that of a Mercurial Cylinder equiponderant to the Aerial one: As if there be a pound of Lead in one Scale, it will not destroy the *equilibrium*, whether what be put in the other be Gold, or Quicksilver, or Wooll, or Feathers, provided its weight be just a pound.

In the next place we may take notice, That the account our Examiner gives us of his *Funiculus* in the tenth Chapter, (where he takes upon him to Explicate it) is much more strange than satisfactory, and not made out by any such parallel operations of Nature, as his Adversaries will not (and may not well do it) dispute the truth of. Whereas the *weight* and *Spring* of the Air may be infer'd from such unquestion'd Experiments as are nor concern'd in our present Controversie. For the gravity of the Air may be manifested by a pair of Scales, and the Spring of it discloses it self so clearly in wind-guns and other Instruments, that our Adversary (as we have already had occasion to inculcate) does not deny it. But to consider his explication of his *Funiculus*, he would have us note two things: First, *Argentum dum replet totum tubum, non* Pag. 38.
mere tangere ejus summitatem (ut primo aspectu videtur)
sed eidem quoq; firmiter adhaerere. Patet hoc (subjoyns he)
experimento illo in primo argumento capitis tertii de tubo utriusq; aper-
to. But what is to be answer'd to this proof may be easily gathered from what we have replyed to that Argument. And to what our Author addes to prove, That the adhesion of the Finger is to the subjacent Mercury, not to the Tube; namely, That *Licet illud tubi orificium oleo, aliave materia ad-*
hesionem impediende, inungatur, non minus tamen fir- Page. 38.
miter adhaerebit digitus quam prius; an Answer may be drawn from the same place: nor perhaps will his reasoning much satisfy those who consider that bodies by friction may easily enough be made stick together; as much as in our case the Tube

and Finger do, notwithstanding one of them is anoynted with Oyle, and that this adhesion of the Finger to the Tube is to be met with in cases where the Surface of the included Quicksilver is not contiguous to the Finger, but many Inches below. As for what he addes concerning the reason why Water and Quicksilver ascend by suction, we have already taught what is to be answered to it, by ascribing that ascension to the pressure of the external Air: without any need of having recourse to a *Puniculus*; or imagining with him in this place, That because nothing besides the Water or Quicksilver can in such cases succeed the Air, (which yet is not easie to be prov'd in reference to a thin *Æthereal substance*) therefore, *Partes ipsius aeris* (to use Pag. 40. his expression) *sic tubo incluse (que alias tam facile separantur) nunc tam fortiter sibi invicem agglutinentur, ut validissimam (uti videmus) consociunt catenam, qua non solum aqua, sed ponderosum illud argentum sic in altum trahatur.* Which way of wreathing a little rarefied Air into so strong a rope, how probable it is, I will for a while leave the Reader to judge, and advance to our Author's second *Notandum*, which he thus proposes:

Rarefactionem sive extensionem corporis ad occupandum majorem locum fieri non solo calore, sed etiam distensione seu vi divulsiva: sicut è contra condensatio non solo frigore perficitur, sed etiam compressione, uti innumera passim docent exempla. And 'tis true and obvious, that the condensation of bodies, taking that word in a large sense, may be made as well by compression as cold. But I wish he had more clearly expressed what he means in this place by that Rarefaction, which he sayes is to be made by distension, or a *vis divulsiva*, whereof he tells us there are innumerable instances. For, as far as may be gathered from the three Examples he subjoyns, 'tis onely the Air that is capable of being so extended as his *Hypothesis* requites Quicksilver and even Stones must be. And I know not how it will be proved, that even Air may be thus extended so far, as in the *Magdeburg Experiment*, to fill a place more than two thousand times as big as that it fill'd before. For that the same

same Air in this and his two foregoing Instances does adequately fill more space at one time than another, he proves but by the rushing in of water into the evacuated Glass, and filling it within a little quite full, which he says, is done by the distended Air that contracting it self draws up the water with it. Which Explication how much less likely it is, than that the water is in such cases impell'd up by the pressure of the Atmosphere, we shall anon (when we come to discuss his way of Rarefaction and condensation) have occasion to examine. In the mean time let us consider with him the Explication which, after having promis'd the two above recited Observations, he gives us of his *Funiculus*; *Cum per primum Notandum*

Pag. 41.

argentum ita adhaereat tubi vertici, & per secundum, refectio fiat per meram corporis distensionem, ita rem se habere, ut argentum descendens à vertice tubi affixam ei relinquat superficiem suam extimam sive supremam, eamq; eousq; suo pondere extendat extenuetque, donec facilius sit aliam superficiem similiter relinquere quam priorem illam ulterius extendere: Secundam igitur relinquit, eamq; eodem modo descendendo extendit, donec facilius sit tertiam adhuc separari quam illam secundam extendere ulterius: & sic deinceps, donec tandem vires amplius non habeat superficies sic separandi & extendendi; nempe donec perveniat ad altitudinem digitorum duntaxat 29½. ubi quiescit, ut capite primo dictum est.

Thus far our Examiners Explication: By which 'tis easie to discern, that he is fain to assign his *Funiculus* a way of being produc'd strange and unparallel'd enough. For, not to repeat our Animadversions upon the first of the two *Notandum's*, on which the Explication is grounded, I must demand by what force, upon the bare separation of the Quicksilver and the top of the Tube, the new body he mentions comes to be produc'd; or at least how it appears that the *Mercury* leaves any such thing as he speaks of behind it. For the sense perceives no such matter at the top of the Tube, nor is it necessary to explicate the *Phænomena* as we have formerly seen. It may also be marvell'd at, that the bare weight of the descending *Mercury* should be able to extend a Surface into a Body. And besides, it seems

pre-

precariouſly affirm'd, that there is ſuch a ſucceſſive leaving behind of one Surface after another as is here imagin'd: Nor does it at all appear how, though ſome of the Quickſilver were turn'd into a thin ſubtile ſubſtance, yet that ſubſtance comes to be contriv'd into a *Funiculus* of ſo ſtrange a nature, that ſcarce any weight (for ought appears by his Doctrine) can be able to break it; that contrary to all other ſtrings it may be ſtretched without being made more ſlender; and that it has other very odde properties, ſome of which we ſhall anon have occaſion to mention. As for what our Author ſubjoyns in theſe words, *Eodem itaq;*
 Tag. 43. 44. *fere modo ſeparari videntur hæ ſuperficies ab argento
 decendente, & in tenuiſſimum quendam funiculum
 per decedens pondus extendi, quo per calorem in accenſa candela
 ſeparantur huiusmodi ſuperficies à ſubjecta cera aut ſewo, & in ſub-
 tiliffimam flammam extenuantur. Ubi notatu dignum, quemad-
 modum flamma illa plusquam millies ſine dubio majus ſpatium occu-
 pat, quam antea occupaverat pars illa cære ex qua conſicitur; ita
 prorsus & hic exiſtimandum Funiculum illum plusquam millies
 majus ſpatium occupare quam prius occupaverat illa argenti parti-
 cula ex qua fit exortus: Uti etiam ſine dubio contingit, quando
 talis particula à ſubjecto igne in vaporem convertitur.* Though
 it be the only Example whereby he endeavours to illuſtrate the
 generation of his *Funiculus*, yet (I preſume) he ſcarce expects
 we ſhould think it an appoſite one. For beſides that there here
 intervenes a conſpicuous and powerful Agent, namely, an ac-
 tual Fire to ſever and agitate the parts of the Candle; and be-
 ſides that there is a manifeſt waſting of the Wax or Tallow turn'd
 into flame; beſides theſe things, I ſay, we muſt not admit
 that the Fuel when turn'd into a flame does really fill (I ſay,
 not, with our Author) more than a thouſand times, but ſo
 much as twice more of genuine ſpace than the Wax 'twas made
 of. For it may be ſaid that the flame is little or nothing elſe
 than an aggregate of thoſe Corpufcles which before lay upon
 the upper ſuperficies of the Candle, and by the violent heat
 were divided into minuter particles, vehemently agitated and
 brought from lying as it were upon a flat to beat off one ano-
 ther

ther, and make up about the Wick such a figure as is usual in the flame of Candles burning in the free Air. Nor will it necessarily follow, that the space which the flame seems to take up should contain neither Air nor *Æther*, nor any thing else, save the parts of that flame, because the eye cannot discern any other body there: For even the smoke ascending from the snuff of a newly-extinguish'd Candle appears a dark pillar, which to the eye at some distance seems to consist of smoke; when as yet there are so many Aerial and other invisible Corpuscles mingled with it, as if all those parts of smoke that make a great show in the Air were collected and contiguous, they would not perhaps amount to the bigness of a Pins head, as may appear by the great quantity of streams that in Chymical Vessels are wont to go to the making up of one drop of Spirit. And therefore it does not ill fall out for our turn, that the Examiner, to enforce his former Example, alledges the turning of a particle of Quicksilver into vapour, by putting fire under it: for if such be the Rarefaction of *Mercury*, 'tis not at all like to make such a *Funiculus* as he talks of, since those Mercurial Fumes appear by divers Experiments to be *Mercury* divided and thrown abroad into minute parts, whereby though the body obtain more of Surface than it had before, yet it really fills no more of true and genuine space; since if all the particular little spaces fill'd by these scatter'd Corpuscles were reduc'd into one, (as the Corpuscles themselves often are in Chymical Operations) they would amount but to one total space, equal to that of the whole *Mercury* before rarefaction. But these Objections against this Explication are not all that I have to say against our Adversaries *Funiculus* it self.

For I farther demand how the *Funiculus* comes by such hooks or grapple-irons, or parts of the like shape, to take fast hold of all contiguous bodies, and even the smoothest, such as Glass, and the calm surface of Quicksilver, Water, Oyle, and other fluids: And how these slender and invisible hooks can not only in the terrestial bodies find an innumerable company of ears or loops to take hold on, but hold so strongly that they are able not alone to lift up a tall Cylinder of that very ponderous metal

tal of Quick-silver, but to draw inwards the sides of strong Glasses so forcibly, as to break them all to pieces. And 'tis also somewhat strange, that Water and other fluid bodies (whose parts are wont to be so easily separable) should, when the *Funiculus* once layes hold on the superficial Corpuscles, presently emulate the nature of consistent bodies, and be drawn up like Masses each of them of an intire piece; though even in the exhausted Receiver they appear by their undulation (when they are stir'd by Bubbles that pass freely through them) and many other signs to continue fluid bodies.

It seems also very difficult to conceive how this extenuated substance should require so strong a spring inward as the Examiner all along his books ascribes to it. Nor will it serve his turn to require of us in exchange an Explication of the Air spring outward, since he acknowledges, as well as we, that it has such a spring. I know, that by calling this extenuated substance a *Funiculus*, he seems plainly to intimate that it has its spring inward, upon the same account that Lute-strings and Ropes forcibly stretch'd have theirs. But there is no small disparity betwixt them: for whereas in strings there is requir'd either wreathing, or some peculiar and artificial texture of the component parts; a rarefaction of Air (were it granted) does not include or infer any such contrivance of parts as is requisite to make bodies Elastical. And if the *Cartesian* Notion of the cause of Springiness be admitted, then our extenuated substance having no Pores to be pervaded by the *materia subtilis* (to which besides our Author also makes Glass impervious) will be destitute of Springiness. And however, since Lute-strings, Ropes, &c. must, when they shrink inwards, either fill up or lessen their Pores, and increase in thickness as they diminish in length; our Examiners *Funiculus* must differ very much from them, since it has no Pores to receive the shrinking parts, and contracts it self as to length, without increasing its thickness. Nor can it well be pretended that this self contraction is done *ob fugam vacui*, since though it should not be made

made, a *Vacuum* would not ensue. And if it be said that it is made that the preternaturally stretch'd Body might restore it self to its natural dimensions: I answer, That I am not very forward to allow acting for ends to Bodies inanimate, and consequently devoid of knowledge; and therefore should gladly see some unquestionable Examples produc'd of Operations of that nature. And however to me, who in Physical enquiries of this nature look for efficient rather than final causes, 'tis not easie to conceive how Air by being expanded (in which case its force (like that of other raref'd Bodies) seems principally to tend outwards, as we see in fired Gun-powder, in *Helipiles*, in warm'd Weather-glasses, &c.) should acquire so prodigious a force of moving contiguous Bodies inwards. Nor does it to me seem very probable, that, when for instance part of a polish'd Marble is extended into a *Funiculus*, that *Funiculus* does so strongly aspire to turn into Marble again. I might likewise with our Author had more clearly explicated, how it comes to pass (which he all along takes for granted) that the access of the outward Air does so much and so suddenly relax the tension of his *Funiculus*; since that being (according to him) a real and Poreless body, 'tis not so obvious how the presence of another can so easily and to so strange a degree make it shrink. But I will rather observe, that 'tis very unlikely that the space which our Adversary would have replenish'd with his *Funicular* substance, should be full of little highly-stretcht strings, that lay fast hold of the surfaces of all contiguous Bodies, and always violently endeavour to pull them inwards. For we have related in our 26. Experiment, that a *Pendulum* being set a moving in our exhausted Receiver, did swing to and fro as freely, and with the string stretch'd as streight, as for ought we could perceive it would have done in the common Air. Nay, the Balance of a Watch did there move freely and nimbly to and fro; which 'tis hard to conceive those Bodies could do, if they were to break through a *medium* consisting of innumerable exceedingly-stretcht strings. On which occasion we might add, that 'tis somewhat strange that these strings, thus cut or broken by the passage

of these bodies through them, could so readily have their parts re-united, and without any more ado be made intire again. And we might also take notice of this as another strange peculiarity in our Author's *Funiculus*, That in this case the two divided parts of each small string that is broken do not, like those of other broken strings, shrink and fly back from one another; but (as we just now said), immediately re-integrate themselves: Whereas, when in the *Torricellian* Experiment the Tube and contain'd Mercury is suddenly lifted up out of the restagnant Quicksilver into the Air, the *Funiculus* does so strangely contract itself, that it quite vanishes; insomuch that the ascending Mercury may rise to the very top of the Tube. These, I say, and divers other difficulties might on this occasion be insisted on; but that, supposing our selves to have mentioned enough of them for once, we think it now more seasonable to proceed to the remaining part of our Discourse.

CHAP. III.

The Aristotelean Rarefaction (proposed by the Adversary) examin'd.

BUT this is not all that renders the Examiner's *Hypothesis* improbable: For, besides those already mentioned particulars, upon whose score it is very difficult to be understood; it necessarily supposes such a Rarefaction and Condensation, as is, I confess, to me, as well as to many other considering persons, *unintelligible*.

For the better discernment of the force of this Objection we must briefly premise, That a Body is commonly said to be raref'd or dilated; (for I take the word in a larger sense than, I know, many others do, for a reason that will quickly appear) when it acquires greater dimensions than the same Body had before; and to be condens'd, when it is reduc'd into less dimensions; that is, into a lesser space than it contain'd before: (as when a dry Sponge being first dipp'd in water swells to a far greater bulk, and then being strongly squeez'd and held compressed, is not only reduced into less room than it had before

before it was squeezed, but into less than it had even before it was wested.) And I must further premise, That Rarefaction (as also Condensation) being amongst the most obvious *Phænomena* of Nature, there are three (and for ought we know but three) ways of explicating it: For, either we must say with the Atomists and Vacuists, that the Corpuscles whereof the rarefied Body consists do so depart from each other, that no other substance comes in between them to fill up the deserted spaces that come to be left betwixt the incontiguous Corpuscles; or else we must say with divers of the ancient Philosophers, and many of the Moderns, especially the *Cartesians*, that these new Intervals produced betwixt the Particles of the rarefied Body are but dilated Pores, replenished, in like manner as those of the tumid Sponge are by the imbibed water, by some subtile *Æthereal* substance, that insinuates it self betwixt the disjoyned Particles: Or, lastly, we must imagine with *Aristotle* and most of his followers, that the self-same Body does not only obtain a greater space in Rarefaction, and a lesser in Condensation, but adequately and exactly fill it, and so when rarefied acquires larger dimensions without either leaving any vacuities betwixt its component Corpuscles, or admitting between them any new or extraneous substance whatsoever.

Now 'tis to this last (and, as some call it, *rigorous*) way of Rarefaction that our Adversary has recourse in his *Hypothesis*: Though this, I confess, appear to me so difficult to be conceived, that I make a doubt whether any *Phænomenon* can be explained by it; since to explain a thing is to deduce it from something or other in Nature more known than it self.

He that would meet with full Discussions of this *Aristotelean* Rarefaction, may resort to the learned writings of *Gassendus*, *Cartesius* and *Maignan*, who have accused it of divers great absurdities: But for my part, I shall at present content my self to make use to my purpose of two or three passages that I meet with (though not together) in our Author himself.

Let us then suppose, that in the *Magdeburg* Experiment he

so often (though I think causlessly enough) urges to prove his *Hypothesis*; let us (I say) for easier considerations sake suppose, that the undilated Air, which (as he tells us) possessed
 Pag. 42. about half an inch of space, consisted of a hundred Corpuscles, or (if that name be in this case disliked) a hundred parts; (for it matters not what number we pitch upon) and 'twill not be denied, but that as the whole parcel of Air, or the Aggregate of this hundred Corpuscles, is adequate to the whole space it fills, so each of the hundred parts, that make it up, is likewise adequately commensurate to its peculiar space, which we here suppose to be a hundredth part of the whole space. This premised, our Author having elsewhere this passage, *Corpore*
 Pag. 160. *occupante locum verbi gratia duplo majorem, necesse est ut qualibet ejus pars locum quoq; duplo majorem occupet*; prompts us to subjoyn, that in the whole capacity of the Globe (which according to him was two thousand times as great as the room possessed by the unexpanded Air) there must likewise be two hundred thousand parts of space commensurate each of them to one of the fore-mentioned hundredth parts of Air; and consequently, when he affirms that that half Inch of Air possessed the whole cavity of the Globe, if we will not admit (as he does not) either Vacuities or some intervening subtile substance in the Interval of the Aërial parts, he must give us leave to conclude, that each part of Air does adequately fill two thousand parts of space. Now that this should be resolutely taught to be not only *naturally possible*, (for we dispute not here of what the Divine Omnipotence can do) but to be really and regularly done in this *Magdeburg* Experiment, will questionless appear very *absurd* to the *Cartesians* and those other Philosophers, who take Extension to be but notionally different from Body, and consequently impossible to be acquir'd or lost without the addition or detraction of Matter; and will, I doubt not, appear *strange* to those other Readers, who consider how generally Naturalists have looked upon Extension as inseparable, and as immediately flowing from matter; and upon Bodies, as having necessary relation to a commensurate space. Nor do I see, if one
 portion

portion of Air may so easily be brought exactly to fill up a space two thousand times as big as that which it did *but* fill before without the addition of any new substance; I see not (I say) why the matter contained in every of these two thousand parts of space may not be further brought to fill two thousand more, and so onwards, since each of these newly-replenished spaces is presumed to be exactly filled with Body, and no Space, nor consequently that which the unrarefied Air replenished, can be more than adequately full. And since, according to our Adversary, not only fluid Bodies, as Air and Quicksilver, but even solid and hard ones, as Marble, are capable of such a Distension as we speak of, why may not the World be made I know not how many thousand times bigger than it is, without either admitting any thing of Vacuity betwixt its parts, or being increased with the addition of one Atome of new matter? Which to me is so difficult to conceive, that I have sometimes doubted, whether in case it could be proved, that in the exhausted Globe we speak of there were no Vacuities within, nor any subtile matter permitted to enter from without, it were not more intelligible to suppose that God had created a new matter to joyn with the Air in filling up the Cavity, than that the self-same Air should adequately fill two thousand spaces, whereof one was exactly commensurate to it even when it was uncompressed. For divers eminent Naturalists, both ancient and modern, believing upon a Physical account the Souls of men to be created and infused, will admit it as intelligible that God does frequently create substances on certain emergent occasions. But I know that many of them will not likewise think it conceivable, that without his immediate interposition an accession of new, real Dimensions should be had without either vacuities or accession of matter.

And indeed when I considered these difficulties and others, that attend the Rarefaction our Examiner throughout his whole Book supposes, and when I found that ever and anon he remits us to what he teaches concerning Rarefaction; I could not but with some greediness resort to the Chapters he addressed me to.

But when I had perused them, I found the Difficulties remained such still, and that 'twas very hard even for a witty man to make more of a subject than the nature of it does bear. Which I say, that by professing my self unsatisfied with what he writes, I may not be thought to find fault with a man for not doing what perhaps is not to be done, and for not making such abstruse Notions plain, as are scarcely (if at all) so much as intelligible. And indeed as he has handled this subject modestly enough, so in some places his Expressions are to me somewhat dark; which I mention, not to impute it as a Crime in him, that he wrote in a diffident and doubtfull strain of so difficult a matter, but to excuse my self if I have not always guessed aright at his meaning.

The things he alledges in favour of the Rarefaction he would persuade are two: The one, That the *Phænomena* of Rarefaction cannot be explicated either by Vacuities or the subingression of an *Æthereal* Substance; and the other, That there are two ways of explicating the rigorous Rarefaction he contends for.

His Objections against the *Epicurean* and *Cartesian* ways of making out Rarefaction are some of them more plausible than most of those that are wont to be urged against them; yet not such as are not capable enough of Answers. But whilst some of the passages appeared easie to be replied to by the Favourers of the *Hypothesis* they oppose, before I had fully examined the rest, chancing to mention these Chapters to an ingenious Man, hereafter to be further mentioned in this Treatise; he told me he had so far considered them more than the rest of the book, that he had thought upon some *Hypotheses*, whereby the *Phænomena* of Rarefaction might be made out either according to the Vacuists, or according to the *Cartesians*, adding, that he had also examined the Instance our Adversary pretends to be afforded him of his Rarefaction by what happens in the *Rota Aristotelica*. Wherefore being sufficiently distressed by Avocations of several sorts, and being willing to reserve the Declaration of my own thoughts concerning the manner of Rarefaction and Condensation for another Treatise, I shall refer the Reader to

the ingenious Conjectures about this Subject, which the Writer of them intends to annex to the present Discourse; and only add in general, That whereas the Examiner's Argument on this occasion is, That his way of Rarefaction must be admitted, because neither of the other two can be well made out, his Adversaries may with the same reason argue that one of theirs is to be allowed, since his is incumbered with such manifest difficulties. And they may enforce what they say by representing, that the inconveniences that attend his *Hypothesis* about Rarefaction are insuperable, arising from the unintelligible nature of the thing it self; whereas those to which the other ways are obnoxious, may seem to spring but from mens not having yet discovered what kind of Figures and Motions of the small Particles may best qualifie them to make the Body that consists of them capable of a competent expansion.

After our Author's Objections against the two ways of Rarefaction proposed, the one by the *Vacuists*, and the other by the *Cartesians* and others, that admit the solidest Bodies, and even Glass it self, to be pervious to an *Æthereal* or subtile matter; he attempts to explicate the manner by which that rigorous Rarefaction he teaches is perform'd: and having premised, that the Explication of the way how each part of the rarefy'd Body becomes extended, depends upon the quality of the parts into which the Body is ultimately resolv'd; and having truly observ'd, that they must necessarily be either really indivisible, or still endlessly divisible; he endeavours to explicate the *Aristotelean* Rarefaction according to those two *Hypotheses*. But, though he thus propose two ways of making out his Rarefaction; yet besides that they are irreconcilable, he speaks of them so darkly and doubtfully, that it seems less easie to discern which of the two he would be content to stick to, than that he himself scarce acquiesces in either of them.

And, first, having told us how Rarefaction may be explain'd, in case we admit Bodies to be divisible *in infinitum*, he does himself make such an Objection against the infinity of parts in a *continuum*, as he is fain to give so obscure an Answer to,

that

that I confess I do not understand it; and presume, that not only the most part of unprejudiced Readers will as little acquiesce in the Answer as I do; but even the Author himself will not marvel at my confession, since in the same place he acknowledges the Answer to be somewhat *obscure*, and endeavours to excuse its being so, because in that *Hypothesis* it can scarce be otherwise.

Wherefore I shall only add on this occasion, that 'tis not clear to me, that even such a divisibility of a *continuum* as is here supposed would make out the Rarefaction he contends for. For, let the integrant parts of a *continuum* be more or less finite or infinite in number, yet still each part, being a corporeal substance, must have some Particle of space commensurate to it; and if the whole Body be rarefied, for instance, to twice its former bigness, then will each part be likewise extended to double its former dimensions, and fill both the place it took up before, and another equal to it, and so two places.

The second Argument alledged to recommend the hitherto-mentioned way of explicating Rarefaction is, That many learned Men, amongst whom he names two, *Aquinas* and *Suarez*, have taught that the same corporeal thing may naturally be, and *de facto* often is, in the souls of Brutes really indivisible and virtually extended. But, though I pay those two Authors a just respect for their great skill in Scholastical and Metaphysical learning; yet the Examiner cannot ignore, that I could make a long Catalogue of Writers, both ancient and modern, at least as well vers'd in natural Philosophy as *Saint Thomas* and *Suarez*, who have *some* of them in express words denied this to be naturally possible; and *others* have declared themselves of the same judgment by establishing principles, with which this Conceit of the virtual extension of the indivisible Corpuscles is absolutely inconsistent. And though no Author had hitherto opposed it, yet I, that dispute not what *this* or *that* man thought, but what 'tis rational to think, should nevertheless not scruple to reject it now; and should

not doubt to find store of the best Naturalists of the same opinion with me, and perhaps among them the Examiner himself, who (however this acknowledgment may agree with the three following Chapters of his book) tells us, (*pag. 160.*) that *Juxta probabiliorē sententiā hujusmodi virtualis extensio rei corporeæ concedenda non est, utpote soli rei spirituali propria.*

But to conclude at length this tedious Enquiry into the Aristotelean way of Rarefaction, (which is of so obscure a nature that it can scarce be either proposed or examined in few words) I will not take upon me resolutely to affirm which of the two ways of explicating it (by Atomes or by Parts infinitely divisible) our Author declares himself for. But which of them soever it be, I think I have shown that he has not intelligibly made it out: And I make the less scruple to do so, because he himself is so ingenuous as (at the close of his discourse of the two ways) to speak thus of the Opinion he prefers; *Præstat communi*

& receptæ hactenus in Scholis sententiæ insistere, quæ licet Pag 169.
difficultates quidem non clarè solvat, iis tamen aperte non succumbit.

So that in this discourse of Rarefaction, to which our Author has so often in the foregoing part of the Book referred us, as that which should make good what there seemed the most improbable; he has but instead of a probable Hypothesis needlessly rejected, substituted a Doctrine which himself dares not pretend capable of being well freed from the difficulties with which it may be charged; though I doubt not but other Readers, especially Naturalists, will think he has been very civil to this obscure Doctrine, in saying that *Difficultatibus non aperte succumbit.*

As for the other way of explicating Rarefaction, namely, by supposing that a body is made up of parts indivisible; he will not, I presume, deny, but that the Objections we formerly made against it are weighty. For according to this Hypothesis (which one would think he prefers) since he makes use of it in the three or four last Chapters of his Book) *Necessarium*
est statuendum esse (says he) unam eandemque partem poni Pag. 169.
in duplici loco, & integrato. Cum enim indivisibilis sit, locumque occupet majorem, quam prius, necesse est ut tota sit in quolibet parte

to totius loci, five ut per totum illud spatium virtualiter extendatur. So that when hein the very next Page affirms, that by this virtual extension of the parts, the Difficulties that have for so many Ages troubled Philosophers may be easily solved, he must give me leave (who love to speak intelligibly, and not to admit what I cannot understand) to desire he would explain to me what this *extensio virtualis* is, and how it will remove the Difficulties that I formerly charged upon the *Aristotelean* Rarefaction. For the easier consideration of this matter, let us resume what we lately supposed, namely, that in the *Magdeburgick* Experiment the half Inch of undilated Air consisted of a hundred Corpuscles; I demand how the indivisibility of these Corpuscles will qualifie them to make out such a Rarefaction as the Author imagines. For what does their being indivisible do in this case, but make it the less intelligible how they can fill above a hundred parts of space? 'Tis easie to foresee he will answer, That they are virtually extended. But not here to question how their indivisibility makes them capable of being so; I demand, whether by an Atoms being virtually extended, its corporeal substance do really (I mean adequately) fill more space than it did before, or whether it do not: (for one of the two is necessary.) If it do, then 'tis a true and real, and not barely a virtual extension. And that such an extension will not serve he turn, what we have formerly argued against the *Peripatetick* Rarefaction will evince; and our Adversary seems to confess as much, by devising this virtual extension to avoid the inconveniences to which he saw his Doctrine of Rarefaction would otherwise plainly appear expos'd. But if it be said, That when an Atom is virtually extended, its corporeal substance fills no more space than before: This is but a Verbal shift, that may perhaps amuse an unwary Reader, but it will scarce satisfy a considering one. For I demand how that which is not a substance can fill place; and how this improper and but Metaphorical Extension will save the *Phænomena* of Rarefaction: as how the half Inch of Air at the top of the fore-mentioned Globe shall without a corporeal extension

extension fill the whole Globe of two thousand times its bigness when the water is suck'd out of it, and act at the lower part of the Globe. Which last Clause I therefore add, because not only our Author teaches (pag. 91. and 92.) that the whole Globe was filled with a certain thin substance, which by its contraction violently snatch'd up the water into which the neck of the Glas was immers'd; but in a parallel case he makes it his grand Argument to prove, that there is no *Vacuum* in the deserted part of the Tube in the *Torricellian* Experiment, Chap. 3. That the attraction of the Finger cannot be performed but by some real Body. Wherefore till the Examiner do intelligibly explain how a virtual Extension, as it is opposed to a corporeal, can make an Atome fill twice, nay, two thousand times more space than it did before; I suppose this device of virtual extension will appear to unbiass'd Naturalists but a very unsatisfactory evasion.

Two Arguments indeed there are which our Adversary offers as proofs of what he teaches. The first is, That they commonly teach in the Schools, that at least *divinitus* (as he speaks) such a thing as is pleaded for may be done, and that consequently it is not repugnant to the nature of a body. But, though they that either know me, or have read what I have written about matters Theological, will, I hope, readily believe, that none is more willing to *acknowledge* and *venerate* Divine Omnipotence; yet in some famous Schools they teach, that it is contrary to the nature of the thing. And that men who think so, and consequently look not upon it as an object of Divine Omnipotence, may (whatever he here say) without *impiety* be of a differing mind from him about the possibility of such a Rarefaction as he would here have, our Author may perchance think fit to grant, if he remember that he himself says a few Pages after, *Cum tempus sit Ess. essentialiter successivum, ita ut ne divinitus quidem possint duae ejus partes simul existere, &c.* Pag. 175. But, not now to dispute of a power that I am more willing to adore than question, I say, that our Controversie is not what God can do, but about what can be done by *Natural Agents*, not

elevated above the sphere of Nature. For though God can both create and annihilate, yet Nature can do neither: and in the judgment of true Philosophers I suppose our *Hypothesis* would need no other advantage to make it be preferred before our Adversaries, than that in ours things are explicated by the ordinary course of Nature, whereas in the other recourse must be had to miracles.

But though our Author's way of explicating *Rarefaction* be thus improbable, yet I must not here omit to take notice, that his *Funiculus* supposes a *Condensation* that to me appears incumbered with no less manifest difficulties. For, since he teaches that a body may be condens'd without either having any vacuities for the compressed parts to retire into, or having Pores filled with any subtile and yielding matter that may be squeez'd out of them; it will follow, that the parts of the Body to be condens'd do immediately touch each other: which supposed, I demand how Bodies that are already contiguous can be brought to farther Approximations without penetrating each other, at least in some of their parts. So that I see not how the Examiners Condensation can be perform'd without *penetration of dimensions*. A thing that Philosophers of all Ages have looked upon as by no means to be admitted in Nature. And our Author himself speaks somewhere at the same rate, where to the Question, Why the walls that inclose fired Gun-powder must be blown asunder? Respondens (says he) *hac omnia inde accipere, quod pulvis ille sic accensus & in flammam conversus, longe majus spatium nunc occupet quam prius. Unde fit, ut cum totum cubiculum antea fuerit plenissimum, disrumpantur sic parietes, ne detur corporum penetratio.* In the *Magdeburgick Experiment* he tells us (as we have heard already) that the whole capacity of the Globe is filled with an extremely-thin body. But not now to examine how properly he calls that a *rare* body, which according to him intercepts neither Pores nor any heterogeneous substance, the greater or lesser absence of which makes men call a Body more or less *dense*; not to insist on this, I say, let us consider, that before the admission of water into the

the exhausted Globe there was, according to him, two thousand half Inches of a substance, which, however it was produc'd or got thither, ~~was~~ a true and real Body; and that after the admission of the water there remained in the same Globe, besides the water that came in, no more than one half Inch of body. Since then our Author does not pretend (which if he did, might be easily disproved) that the one thousand nine hundred ninety nine half Inches of Matter, that now appear no more, traversed the body of Water; since he will not allow that it gets away through the Pores of the Glass, I demand, what becomes of so great a quantity of Matter? For that 'tis annihilated I suppose he is too rational a man to pretend, (nor, if he should, would it be at all believ'd) and to say, that a thousand and so many hundred parts of Matter should be retir'd into that one part of space that contains the one half Inch of Air, is little less incredible: For that space was suppos'd perfectly full of body before, and how a thing can be more than perfectly full, who can conceive? To dispatch: According to our Author's way of Condensation, two, or perhaps two thousand, Bodies may be croud-ed into a space that is adequately fill'd by one of them apart. And if this be not penetration of Dimensions, I desire to be informed what is so; and till then I shall leave it to any unprepossess'd Naturalist to judge, whether an *Hypothesis* that needs suppose a thing so generally concluded to be impossible to Nature, be *probable* or not; and whether to tell us that the very same parcel of Air, that is now without violence contain'd in half an Inch of space, shall by and by fill two thousand times as much room, and presently after shrink again into the two thousandth part of the space it newly possess'd, be not to turn a Body into a Spirit, and, confounding their Notions, attribute to the former the discriminating and least easily conceivable properties of the later. And this Argument is, I confess, with me of that weight, that this alone would keep me from admitting the Examiners *Hypothesis*: Yet if any happier Contemplator shall prove so sharp-sighted, as to devise and clearly propose a way of making the Rarefaction and Condensa-

tion hitherto argued against, intelligible to me, he is not like to find me obstinate. Nor indeed is there sufficient cause why his succeeding in that attempt should make our Adversaries *Hypothesis* preferable to ours, since that would not prove it either *necessary*, or so much as *sufficient*, but only answer *some* of the Arguments that tend to prove 'its not intelligible. And that we have other Arguments on our side than those that relate to Rarefaction and Condensation, may appear partly by what has been discours'd already, and partly by what we have now to subjoin.

CHAP. IV.

A Consideration (pertinent to the present Controversie) of what happens in trying the Torricellian and other Experiments, at the tops and feet of Hills.

THere remain then yet a couple of Considerations to be oppos'd against the Examiners *Hypothesis*, which, though the past Discourse may make them be look'd upon as needless, we must not pretermitt, because they contain such Arguments as may not only be employed against our Adversaries Doctrine, but will very much tend to the confirmation of ours.

I consider then further, that the *Hypothesis* I am opposing, being but a kind of Inversion of ours, and supposing the spring or motion of Restitution in the Air to tend inwards, as according to us it tends outwards; it cannot be, that if the supposition it self were (what I think I have prov'd it is not) true, many of the *Phænomena* would be plausibly enough explicable by it: the same motions in an intermediate body being in many cases producible alike, whether we suppose it to be thrust or drawn; provided both the endeavours tend the same way. But then we may be satisfied whether the effect be to be ascribed to Pulsion or to Traction, (as they commonly speak, though indeed the later seems reducible to the former) if we can find out an Experiment wherein there is reason such an effect should follow, in case Pulsion be the cause inquired after, and not in case it be Traction. And such an *Experimentum Crucis* (to speak with our *Illu-*
strious

serius Verulam) is afforded us by that noble Observation of Monsieur *Paschal*, mentioned by the famous *Pecquet*, and out of him by our Author: namely, that the *Torriceillian* Ex- Pag. 66.
periment being made at the foot and in divers places of a very high Mountain, (of the altitude of five hundred fathom or three thousand foot) he found, that after he had ascended a hundred and fifty Fathom, the Quicksilver was fallen two Inches and a quarter below its station at the Mountains foot; and that at the very top of the Hill it had descended above three Inches below the same wonted station. Whence it appears that the Quicksilver being carried up towards the top of the *Atmosphere*, falls down the lower, the higher the place is wherein the observation is made: of which the reason is plain in our *Hypothesis*, namely, that the nearer we come to the top of the *Atmosphere*, the shorter and lighter is the Cylinder of Air incumbent upon the stagnant *Mercury*; and consequently the less weight of Cylindrical *Mercury* will that Air be able to counterpoise and keep suspended. And since this notable *Phænomenon* does thus clearly follow upon ours, and not upon our Adversaries *Hypothesis*; this Experiment seems to determine the Controversie betwixt them: because in this case the Examiner cannot pretend, as he does in the seventeenth and divers other of our Experiments, that the descent of the Quicksilver in the Tube is caus'd, not by the diminution of the external Airs pressure, but from the preternatural Rarefaction or Distension of that external Air (in the Receiver) when by seeking to restore it self, it endeavours to draw up the stagnant *Mercury*: For in our present case there appears no such forcible Dilatation of that Air, as in many of the *Phænomena* of our Engine he is pleas'd to imagine.

It need therefore be no great wonder, if his Adversaries do, as he observes, make a great account of this Experiment, to prove that the *Mercury* is kept up in the Tube by the resistance of the external Air. Nor do I think his Answers to the Argument drawn from hence will keep them from thinking it cogent. For to an Objection upon which he takes notice that they lay so much stress, he replies but two things; which

which neither singly nor together will bear amount to a satisfactory Answer.

And, *First*, he questions the truth of the Observation it self; because having made trial in a low Hill, the event did no ways answer his expectation. But though, in stead of disapproving, I am willing to commend his Curiosity; to make the Experiment himself, and especially since 'twas both new and important; and though also I like his Modesty, in rather suspecting some mistake in the manner of the Observation, than that the Experimenters did not sincerely deliver it: yet, since there must be an Error somewhere, I must rather charge it upon the Examiners observation (I say his Observation, not his want of sincerity) than upon Monsieur *Paschal's*. For besides the commendations that the learned *Gassendus*, who relates the Experiment, gives to that ingenious Gentleman (Monsieur *Paschal*) by whose direction he supposes it to have been try'd: the same *Gassendus* relates, that the like Observation was five

Gassendus. Tr. p. 211. times repeated, partim intra facellum, partim dère libero, & nunc quidem flante, nunc silente vento.

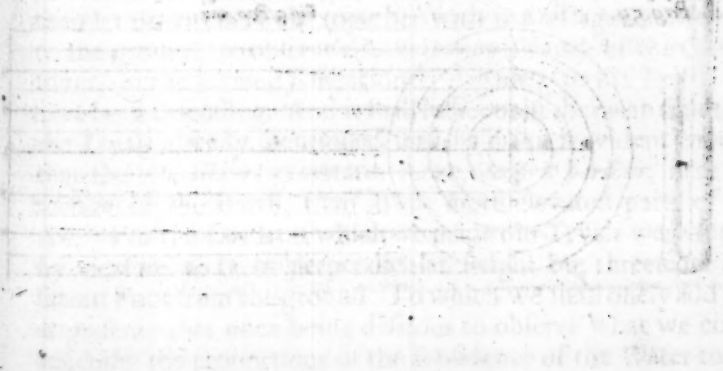
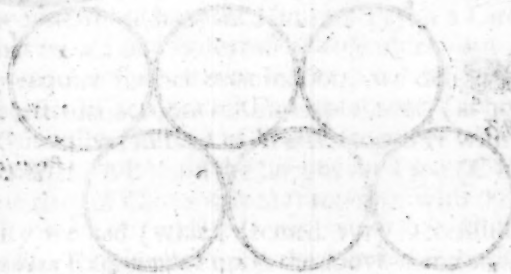
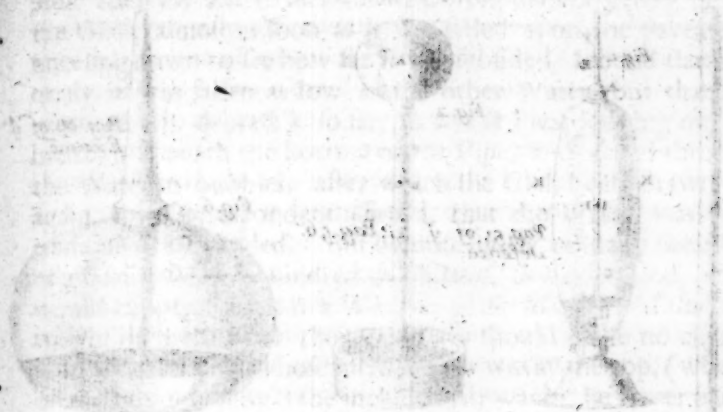
Which circumstances sufficiently argue the Diligence where with the Experiment was try'd in *Anvergne*. Especially since I can confirm these Observations by two more made on distant Hills in *England*: the one of which I procur'd from that known *Virtuoso* Mr. *J. Ball*, whom I desir'd to make the Experiment at a Mountain in *Devonshire*, on the side whereof he dwells; and the other made in *Lancashire* by that ingenious Gentleman Mr. *Richard Townley*. Both which Observations, since I have mentioned them at large in the *Appendix* to the *Physico-Mathematical Transactions* shall not now repeat; contenting my self to observe to our present purpose, that however the proportion of the Descent of the Quicksilver may vary, according to the differing consistence and other accidents of the neighbouring Air in the particular places and times of the Experiments being made, yet all Observations agree in this, That nearer the top of the Atmosphere the Quicksilver falls lower than it does further from it. To all this I shall add two things that will very much confirm our

Hypothesis

Hypothesis. The one is, that the freshly-nam'd Mr. Townley, and divers ingenious Persons that assisted at the Tryal, be-thought themselves of so making the *Torricellian* Experiment at the top of the Hill, as to leave a determinate quantity of Air in the Tube, before the mouth of it was open'd under the vessell'd *Mercury*; and taking notice how low such a quantity of that Air depressed the *Mercurial* Cylinder, they likewise observ'd, that at the Mountains foot the included Air was not able to depress the *Quicksilver* so much. Whence we inferre, that the Cylinder of Air at the top of the Hill being shorter and lighter, did not so strongly press against the included Air, as did the ambient Air at the bottom of the Hill, where the *Aëreal* Cylinder was longer and heavier.

But because that though Experiments made in very elevated places are noble ones, and of great importance in the Controversies about the Air, yet there are but very few of those that are qualified to make Experiments of that Nature, who have the opportunity of making them upon high Mountains; we did with the assistance of an ingenious man attempt a Tryal, wherein we hoped to find a sensibly-differing Weight of the *Atmosphere*, in a far less height than that of an ordinary Hill. But in stead of a common Tube we made use of a kind of Weather-glass, that the included Air might help to make the event notable, for a reason to be mentioned ere long; and in stead of *Quicksilver* we employ'd common Water in the Pipe belonging to the Weather-glass, that small changes in the Weight or resistance of the *Atmosphere* in opposition of the included Air might be the more discernable. The Instrument we made use of consisted onely of a Glass with a broad Foot and a narrow Neck (A B) and a slender Glass-Pipe (C D) open at both ends: which Pipe was so placed, that the bottom of it did almost, but not quite, reach to the bottom of the bigger Glass (A B) within whose Neck (A) it was fastned with a close cement, that both kept the Pipe in its place, and hindred all communication betwixt the inward (I I) and outward (K K) Air, save by the cavity of the Pipe (C D). Now we chose

this Glas (A B) more than ordinary capacious, that the effect of the dilatation of the included Air (I I) might be the more conspicuous. Then conveying a convenient quantity of Water (H H) into this Glas, we carried it to the Leads of the lofty Abby-Church at *Westminster*, and there blew in a little Air to raise the Water to the upper part of the Pipe, that being above the Vessel (A B) we might more precisely mark the several stations of the Water than otherwise we could. Afterward having suffered the Glas to rest a pretty while upon the Lead, that the Air (I I) within might be reduc'd to the same state, both as to coldness and as to pressure, with (K K) that without, having marked the station of the Water (F), we gently let down the Vessel by a long string to the foot of the Wall, where one attended to receive it; who having suffer'd it to rest upon the ground, cry'd to us that it was subsided about an Inch below the mark (F) we had put: whereupon having order'd him to put a mark at his second station of it (E), we drew up the Vessel again; and suffering it to rest a while, we observ'd the Water to be re-ascended to or near the first mark (F), which was indeed about an Inch above (E) the other. And this we did that Evening a second time with almost a like success: though two or three dayes after, the wind blowing strongly upon the Leads, we found not the Experiment to succeed quite so regularly as before; yet the Water alwayes manifestly fell lower at the foot of the Wall than it was at the top: which I see no cause to ascribe barely to the differing temperature of the Air above and below, as to Heat and Cold, since according to the general estimate, the more elevated Region of the Air is, *ceteris paribus*, colder than that below, which would rather check the greater expansion of the included Air at the top of the Leads than promote it. But the better to avoid mistakes and prevent Objections, we thought fit to try the Experiment within the Church, and got into a Gallery of the same height with the Leads: but the upper part of the Pipe being casually broken off, we thought fit to order the matter so, that the surface (G) of the remaining Water in the Pipe should be about



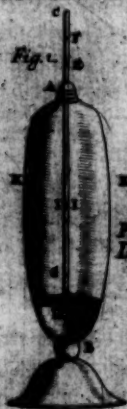


Fig. 1.

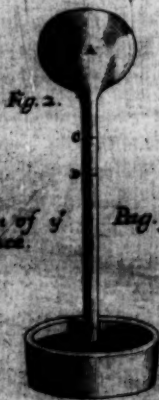


Fig. 2.

Fig. 3. of 1
Deference.

Fig. 56.



Fig. 6.

Fig. 67 of 1
Exame.

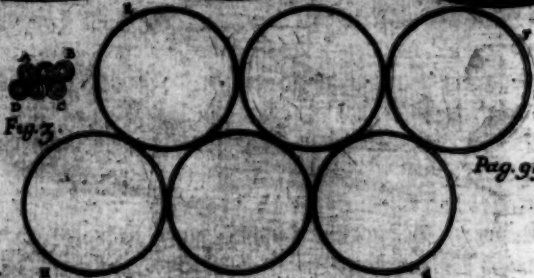
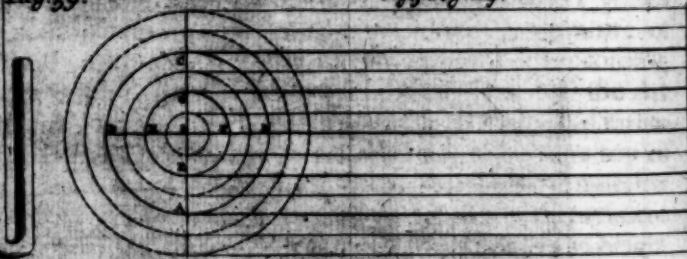


Fig. 3.

Fig. 95.

Fig. 4.
Fig. 59.

Fig. 5. Fig. 104.



an Inch higher than the surface of the Water in the Vessel. And then my above-mentioned Correspondent letting down the Glass, almost as soon as it was settled upon the pavement, kneeling down to see how far it was subsided, I found that not onely it was fallen as low as the other Water, but that the outward Air deprest it so far, as whilst I was looking on, to break in beneath the bottom of the Pipe, and ascend through the Water in bubbles; after which the Glass being drawn up again, my Correspondent affirm'd, that the Water was very manifestly re-ascended. But because by the unlucky breaking of a Glass, we were hindred to observe, as we designed, what would happen as well in a Weather-glass, so contriv'd that the weight or pressure of the *Atmosphere* should make no change in it, as in another whose included Air was at the top, (whereas in that we employ'd the included Air was in the lower part,) and because there happened in our Tryals a Circumstance or two that seem'd not so devoid of difficulties, but that we think it may require further examination, we design to set down a more particular account of this Experiment, (as how it succeeds with Quicksilver instead of Water, together with the capacity of the Vessel (A B) and the bore of the Pipe (C D) with some other variety of Circumstances) together with the event of the curiosity we had (which seem'd very successfull) to try the *Torricellian* Experiment upon the above-mentioned Leads, and then let down the Tube together with the restagnant *Mercury* to the ground, to observe the increasing altitude of the Quick-silver, in the formerly-mentioned *Appendix* to the Epistle we have been defending. And it shall suffice us in the mean time that the Tryals already mentioned seem to make it evident enough that the *Atmosphere* gravitates more, *ceteris paribus*, near the surface of the Earth, than in the more elevated parts of the Air. For the Leads on which we made our Tryals were found by measure to be in perpendicular height but threescore and fifteen Foot from the ground. To which we shall onely add this at present, that once being desirous to observe what we could touching the proportions of the subsidence of the Water to the

height of its several stations from the ground, purposely carrying down the Vessel so as not considerably to heat it, from the Leads down the stairs to a little window that we guesst to be almost half way to the bottom, we there perceived the water to have already subsided about a Barly-Corns length, notwithstanding that probably in spite of our care, the Vessel were a little warmed by the heat of his body that carried it, since by that time we were come to the foot of the Wall, the Water stood almost at the highest mark; but after the Vessel was suffered to rest a while, it relapsed by degrees to the lowest. And thus much for the first of the things I had to represent in favour of our Doctrine.

The other Particular I shall mention for confirmation of our *Hypothesis*, is that Experiment (which, though it be needless, seems yet more cogent and proper to prevent Evasions) made by the same *Monsieur Paschal*, of carrying a weakly-blown Foot-ball from the bottom to the top of an high Mountain. For that Foot-ball swell'd more and more, the higher it was carried, so that it appeared as if it were full blown at the top of the Mountain, and gradually growing lank again, as it was carried downwards; so that at the foot of the Hill it was flaccidas before. This, I say, having thus happened, we have here an Experiment to prove our *Hypothesis*, wherein recourse cannot be had to any forcibly and preternaturally distended Body, such as that is pretended to be which remains in the deserted space of the Tube in the *Torricellian* Experiment.

The other thing which the Examiner alledges against our Argument from *Monsieur Paschal's* Tryals, is, that supposing it to be true, yet it cannot thence be inferr'd, that the subsidence of the *Mercury* at the top of the Hill proceeded from the *Atmospherical* Cylinder's being there lighter and less able to sustain the Quicksilver. *Sed dici potest* (says he) *ideo sic*

Page 63. *in vertice Montis magis descendisse, quod ibidem esset Aura frigidior, aut ex alio Temperamento hujusmodi descensum causante.* But this solution will not serve the turn: For the coldness of the ambient Air (which yet the Experimenters take not notice

notice of) would rather contract the rarefied substance within the Tube, and so draw up the *Mercury* higher, as our Author himself teaches us, that 'tis from the shrinking of the *Funiculus* occasion'd by the cold that the Water in *Thermometers* ascends in cold weather. And whereas the onely Pag. 50. proof he addes of so improbable an Explication is taken from our eighteenth Experiment, wherein we relate, that sometimes the Quicksilver did sensibly fall lower in colder than in far less cold weather: I answer, that this eighteenth Experiment will scarce make more for him than against him: For, as I there take notice that the Quicksilver descended in cold weather, so it sometimes descended likewise in hot weather, and rose in cold: And 'tis very strange, that in all the Observations made, in differing Countries and at differing times, it should still so happen that the *Mercurial* Cylinder should be shorter near the top of the *Atmosphere* than further from it; if the resistance of the outward Air have nothing to do with the keeping it suspended. And 'tis yet more strange, that the foot-ball should in like manner grow turgid and flaccid, according as it is carried into places where it has a shorter or longer Pillar of Air incumbent on it.

I was going to proceed to what remains of this second Part of our Treatise, But that since I begun this Chapter casually meeting with an Experiment lately sent in a Letter to a very Ingenious * Acquaintance of his and mine by a very Industrious Physician * (who is said to have had the curiosity to try over again many of the Experiments of our Engine) and finding it very proper to confirm our newly related Experiment made at *Westminster*, and to be of such a nature as we have not in this part of *England* the opportunity to try the like, for want of Hills high enough, I shall (according to the permission given me) insert it in this place. And the rather, that as the Mountains have by the Tryals made on them of the *Torricellian* Experiment, afforded us a noble proof of the weight of the Air; so they may afford us one of its Spring;

* Mr. Croon one of the learned Professors of *Gresham College*.

* Dr. Hen. Power.

wherein I hope the *Phænomenon* of the Waters descent will not be ascribed to any attraction made of the Water by the violently-distended outward Air. And because the Experiment was not made by us, but by another, we will set it down in his words, which are these: *This fifteenth of October*

See the second Figure.

1661. we took a Weather-glass AB, of about two foot in length, and carrying it to the bottom of *Hallifax Hill*, the Water stood in the Shank at thirteen Inches above the Water in the Vessel: Thence carrying it thus fill'd, with the whole frame, immediately to the top of the said Hill, the Water fell down to the point D, viz. an Inch and a quarter lower than it was at the bottom of the said Hill; which (as he rightly inferrs) proves the Elasticity of the Air: for the internal Air AC, which was of the same power and extension with the external at the bottom of the Hill, did manifest a greater Elasticity than the Mountain-Air there *, and so

* Probably these or the like words, did manifest Pressure, are here omitted, for the Mountain-Air there seems to have acted rather by its Weight than Elasticity.

extended it self further by CD.

The like Experiment, I hear, the same Ingenious Doctor has very lately repeated, and found the descent of the Water to be greater than before. And though some *Virtuosi* have thought it strange, that in an Hill far inferior to the *Alps* and *Appennines*, so short a Cylinder of so light a liquor as Water should fall so much; yet I see not any reason to distrust upon this ground either His Experiment or Ours (lately mention'd to have been made at *Westminster*;) but rather to wonder the Water fell not more (if the Hill be considerably high:) for their suspicion seems grounded upon a mistake, as if because the Quicksilver in the *Torricellian* Experiment made without purposely leaving any Air in the Tube, would not, at the top of the mention'd Hill, have subsided above an Inch, if so much, the Water, that is near fourteen times lighter, should not fall above a fourteenth part of that space; whereas in the *Torricellian* Experiment, the upper and deserted space of the Tube has little or no Air left in it, but the Correspondent part of the Weather-glass was furnish'd with Air, whose pressure

pressure was little less than that of the *Atmosphere* at the bottom of the Hill; and consequently must be much greater than the pressure of the *Atmosphere* at the top of the Hill, where the *Atmospherical Cylinder's* gravity (upon whose account it presses) must be much diminish'd by its being made much shorter, and by its consisting of an Air less compress'd. And thus much for the first of the two Considerations wherewith I promised to conclude this second part of the present Tract. Onely before I proceed I must in a word desire the Reader to take notice, that though I have here singled out but one of the nine Experiments which the Examiner in the 11. and 12. Chapters reckons up as urg'd by his Adversaries; yet do not thereby declare my acquiescing in his Explications of those *Phænomena*, but onely leave both them and some other things he delivers about Siphons and the *Magdeburg* Experiments, to be discours'd by those that are more concerned to examine them, contenting my self to have sufficiently disproved the *Funiculus* which his Expositions suppose, and cleared the grounds of explicating such Experiments aright.

CHAP. V.

Two new Experiments touching the measure of the Force of the Spring of Air compress'd and dilated.

THE other thing that I would have considered touching our Adversaries *Hypothesis* is, That it is *needless*. For whereas he denies not that the Air has some Weight and Spring, but affirms that it is very insufficient to perform Pag. 11. such great matters as the counterpoising of a *Mercurial Cylinder* of 29. Inches, as we teach that it may: We shall now endeavour to manifest by Experiments purposely made, that the Spring of the Air is capable of doing far more than 'tis necessary for us to ascribe to it, to salve the *Phænomena* of the *Torricellian* Experiment.

We took then a long Glass-Tube, which by a dexterous hand and the help of a Lamp was in such a manner crooked at the bottom, that the part turned up was almost parallel to the rest
of

of the Tube, and the Orifice of this shorter leg of the Siphon (if I may so call the whole Instrument) being Hermetically seal'd, the length of it was divided into Inches, (each of which was subdivided into eight parts) by a streight list of paper, which containing those Divisions was carefully pasted all along it: then putting in as much Quicksilver as served to fill the Arch or bended part of the Siphon, that the Mercury standing in a level might reach in the one leg to the bottom of the divided paper, and just to the same height or Horizontal line in the other; we took care, by frequently inclining the Tube, so that the Air might freely pass from one leg into the other by the sides of the Mercury, (we took (I say) care) that the Air at last included in the shorter Cylinder should be of the same laxity with the rest of the Air about it. This done, we began to pour Quicksilver into the longer leg of the Siphon, which by its weight pressing up that in the shorter leg, did by degrees streighten the included Air: and continuing this pouring in of Quicksilver till the Air in the shorter leg was by condensation reduced to take up but half the space it possess'd (I say, *possess'd*, not *fill'd*) before; we cast our eyes upon the longer leg of the Glas, on which was likewise pasted a list of paper carefully divided into Inches and parts, and we observed, not without delight and satisfaction, that the Quicksilver in that longer part of the Tube was 29. Inches higher than the other. Now that this Observation does both very well agree with and confirm our *Hypothesis*, will be easily discerned by him that takes notice what we teach, and Monsieur *Pascal* and our *English* friends Experiments prove, that the greater the weight is that leans upon the Air, the more forcible is its endeavour of Dilatation, and consequently its power of resistance, (as other Springs are stronger when bent by greater weights.) For this being considered, it will appear to agree rarely-well with the *Hypothesis*, that as according to it the Air in that degree of density and correspondent measure of resistance to which the weight of the incumbent Atmosphere had brought it, was able to counterbalance and resist the pressure of a Mercurial Cylinder of about 29. Inches,

Inches, as we are taught by the *Torricellian* Experiment; so here the same Air being brought to a degree of density about twice as great as that it had before, obtains a Spring twice as strong as formerly. As may appear by its being able to sustain or resist a Cylinder of 29 Inches in the longer Tube, together with the weight of the Atmospheric Cylinder, that lean'd upon those 29 Inches of *Mercury*; and, as we just now inferr'd from the *Torricellian* Experiment, was equivalent to them.

We were hindered from prosecuting the trial at that time by the casual breaking of the Tube. But because an accurate Experiment of this nature would be of great importance to the Doctrine of the Spring of the Air, and has not yet been made (that I know) by any man; and because also it is more uneasy to be made than one would think, in regard of the difficulty as well of procuring crooked Tubes fit for the purpose, as of making a just estimate of the true place of the Protuberant *Mercury's* surface; I suppose it will not be unwelcome to the Reader, to be informed that after some other trials, one of which we made in a Tube whose longer leg was perpendicular, and the other, that contained the Air, parallel to the Horizon, we at last procured a Tube of the Figure express'd in the Scheme; which Tube, though of a pretty bigness, was so long, that the Cylinder whereof the shorter leg of it consisted admitted a list of Paper, which had before been divided into 12 Inches and their quarters, and the longer leg admitted another list of Paper of divers foot in length, and divided after the same manner: then Quicksilver being poured in to fill up the bended part of the Glass, that the surface of it in either leg might rest in the same Horizontal line, as we lately taught, there was more and more Quicksilver poured into the longer Tube; and notice being watchfully taken how far the *Mercury* was risen in that longer Tube, when it appeared to have ascended to any of the divisions in the shorter Tube, the several Observations that were thus successively made, and as they were made set down, afforded us the ensuing Table.

See the 5.
Figure.

A Table of the Condensation of the Air

| A | B | C | D | E |
|----|------------------|------------------|-------------------|-------------------|
| 48 | 12 | 00 | 29 $\frac{1}{8}$ | 29 $\frac{1}{8}$ |
| 46 | 11 $\frac{1}{2}$ | 01 $\frac{1}{8}$ | 30 $\frac{1}{8}$ | 30 $\frac{1}{8}$ |
| 44 | 11 | 02 $\frac{1}{4}$ | 31 $\frac{1}{4}$ | 31 $\frac{1}{4}$ |
| 42 | 10 $\frac{1}{2}$ | 04 $\frac{1}{4}$ | 33 $\frac{1}{4}$ | 33 $\frac{1}{4}$ |
| 40 | 10 | 06 $\frac{1}{2}$ | 35 $\frac{1}{2}$ | 35 $\frac{1}{2}$ |
| 38 | 9 $\frac{1}{2}$ | 07 $\frac{1}{2}$ | 37 $\frac{1}{2}$ | 36 $\frac{1}{2}$ |
| 36 | 9 | 10 $\frac{1}{2}$ | 39 $\frac{1}{2}$ | 38 $\frac{1}{2}$ |
| 34 | 8 $\frac{1}{2}$ | 12 $\frac{1}{2}$ | 41 $\frac{1}{2}$ | 41 $\frac{1}{2}$ |
| 32 | 8 | 15 $\frac{1}{2}$ | 44 $\frac{1}{2}$ | 43 $\frac{1}{2}$ |
| 30 | 7 $\frac{1}{2}$ | 17 $\frac{1}{2}$ | 47 $\frac{1}{2}$ | 46 $\frac{1}{2}$ |
| 28 | 7 | 21 $\frac{1}{2}$ | 50 $\frac{1}{2}$ | 50 $\frac{1}{2}$ |
| 26 | 6 $\frac{1}{2}$ | 25 $\frac{1}{2}$ | 54 $\frac{1}{2}$ | 53 $\frac{1}{2}$ |
| 24 | 6 | 29 $\frac{1}{2}$ | 58 $\frac{1}{2}$ | 58 $\frac{1}{2}$ |
| 23 | 5 $\frac{3}{4}$ | 32 $\frac{1}{4}$ | 61 $\frac{1}{4}$ | 60 $\frac{1}{4}$ |
| 22 | 5 $\frac{1}{2}$ | 34 $\frac{1}{2}$ | 64 $\frac{1}{2}$ | 63 $\frac{1}{2}$ |
| 21 | 5 $\frac{1}{4}$ | 37 $\frac{1}{4}$ | 67 $\frac{1}{4}$ | 66 $\frac{1}{4}$ |
| 20 | 5 | 41 $\frac{1}{2}$ | 70 $\frac{1}{2}$ | 70 $\frac{1}{2}$ |
| 19 | 4 $\frac{3}{4}$ | 45 $\frac{1}{4}$ | 74 $\frac{1}{4}$ | 73 $\frac{1}{4}$ |
| 18 | 4 $\frac{1}{2}$ | 48 $\frac{1}{2}$ | 77 $\frac{1}{2}$ | 77 $\frac{1}{2}$ |
| 17 | 4 $\frac{1}{4}$ | 53 $\frac{1}{4}$ | 82 $\frac{1}{4}$ | 82 $\frac{1}{4}$ |
| 16 | 4 | 58 $\frac{1}{2}$ | 87 $\frac{1}{2}$ | 87 $\frac{1}{2}$ |
| 15 | 3 $\frac{3}{4}$ | 63 $\frac{1}{4}$ | 93 $\frac{1}{4}$ | 93 $\frac{1}{4}$ |
| 14 | 3 $\frac{1}{2}$ | 71 $\frac{1}{2}$ | 100 $\frac{1}{2}$ | 99 $\frac{1}{2}$ |
| 13 | 3 $\frac{1}{4}$ | 78 $\frac{1}{4}$ | 107 $\frac{1}{4}$ | 107 $\frac{1}{4}$ |
| 12 | 3 | 88 $\frac{1}{2}$ | 117 $\frac{1}{2}$ | 116 $\frac{1}{2}$ |

Added to 29 $\frac{1}{8}$ makes

A.A. The number of equal spaces in the shorter leg, that contained the same parcel of Air diversly extended.

B. The height of the Mercurial Cylinder in the longer leg, that compress'd the Air into those dimensions.

C. The height of a Mercurial Cylinder that counterbalanc'd the pressure of the Atmosphere.

D. The Aggregate of the two last Columns *B* and *C*, exhibiting the pressure sustained by the included Air.

E. What that pressure should be according to the *Hypothesis*, that supposes the pressures and expansions to be in reciprocal proportion.

For the better understanding of this Experiment it may not be amiss to take notice of the following particulars :

1. That the Tube being so tall that we could not conveniently make use of it in a Chamber, we were fain to use it on a pair of Stairs, which yet were very lightsome, the Tube being for preservations sake by strings so suspended, that it did scarce touch the Box presently to be mentioned.

2. The lower and crooked part of the Pipe was placed in a square wooden Box, of a good largeness and depth, to prevent the

the loss of the Quicksilver that might fall aside in the transfusion from the Vessel into the Pipe, and to receive the whole Quicksilver in case the Tube should break.

3. That we were two to make the Observation together, the one to take notice at the bottom how the Quicksilver rose in the shorter Cylinder, and the other to pour in at the top of the longer, it being very hard and troublesome for one man alone to do both accurately.

4. That the Quicksilver was poured in but by little and little, according to the direction of him that observed below, it being, far easier to pour in more, than to take out any in case too much at once had been poured in.

5. That at the beginning of the Operation, that we might the more truly discern where the Quicksilver rested from time to time, we made use of a small Looking-glass, held in a convenient posture to reflect to the eye what we desired to discern.

6. That when the Air was so compress'd, as to be croud'd into less than a quarter of the space it possess'd before, we try'd whether the cold of a Linen Cloth dipp'd in Water would then condense it. And it sometimes seem'd a little to shrink, but not so manifestly as that we dare build any thing upon it. We then tried likewise whether heat would notwithstanding so forcible a compressure dilate it, and approaching the flame of a Candle to that part where the Air was pent up, the heat had a more sensible operation than the cold had before; so that we scarce doubted but that the expansion of the Air would, notwithstanding the weight that oppress'd it, have been made conspicuous, if the fear of unseasonably breaking the Glass had not kept us from increasing the heat.

Now although we deny not but that in our Table some particulars do not so exactly answer to what our formerly-intimated *Hypothesis* might perchance invite the Reader to expect; yet the Variations are not so considerable, but that they may probably enough be ascribed to some such want of exactness as in such nice Experiments is scarce avoidable. But for all

that, till further trial hath more clearly informed me, I shall not venture to determine whether or no the intimated Theory will hold universally and precisely, either in Condensation of Air, or Rarefaction: All that I shall now urge being, That however, the trial already made sufficiently proves the main thing for which I here alledge it; since by it 'tis evident, that as common Air when reduc'd to half its wonted extent, obtained near about twice as forcible a Spring as it had before; so this thus compress'd Air being further thrust into half this narrow room, obtained thereby a Spring about as strong again as that it last had, and consequently four times as strong as that of the common Air. And there is no cause to doubt, that if we had been here furnish'd with a greater quantity of Quicksilver and a very strong Tube, we might by a further compression of the included Air have made it counterbalance the pressure of a far taller and heavier Cylinder of Mercury. For no man perhaps yet knows how near to an infinite compression the Air may be capable of, if the compressing force be competently increas'd. So that here our Adversary may plainly see that the Spring of the Air, which he makes so light of, may not only be able to resist the weight of 29 Inches, but in some cases of above an hundred Inches of Quicksilver; and that without the assistance of his *Funiculus*, which in our present case has nothing to do. And to let you see that we did not (a little above) inconsiderately mention the weight of the incumbent Atmospherical Cylinder as a part of the weight resisted by the imprisoned Air, we will here annex, that we took care, when the Mercurial Cylinder in the longer leg of the Pipe was about an hundred Inches high, to cause one to suck at the open Orifice; whereupon (as we expected) the Mercury in the Tube did notably ascend. Which considerable *Phenomenon* cannot be ascribed to our Examiners *Funiculus*, since by his own confession that cannot pull up the Mercury, if the Mercurial Cylinder be above 29 or 30 Inches of Mercury. And therefore we shall render this reason of it, That the pressure of the incumbent Air being in part taken off by its expanding it self

self into the Suckers dilated Chests the imprison'd Air was thereby enabled to dilate it self manifestly, and repel the Mercury that compress'd it, till there was an equality of force betwixt the strong Spring of that compress'd Air on the one part, and the tall Mercurial Cylinder, together with the contiguous dilated Air, on the other part.

Now, if to what we have thus delivered concerning the compression of Air we add some Observations concerning its spontaneous Expansion, it will the better appear how much the Phenomena of these Mercurial Experiments depend upon the differing measures of strength to be met with in the Air's Spring, according to its various degrees of compression and Laxity. But, before I enter upon this subject, I shall readily acknowledge that I had not reduc'd the trials I had made about measuring the Expansion of the Air to any certain Hypothesis, when that ingenious Gentleman Mr. Richard Townley was pleas'd to inform me, that having by the perusal of my *Physico-Mechanical* Experiments been satisfied that the Spring of the Air was the cause of it, he had endeavour'd (and I wish in such attempts other ingenious men would follow his example) to supply what I had omitted concerning the reducing to a precise estimate how much Air dilated of it self loses of its Elastic force, according to the measures of its Dilatation. He added, that he had begun to set down what occurred to him to this purpose in a short Discourse, whereof he afterwards did me the favour to shew me the beginning, which gives me a just Curiosity to see it perfected. But, because I neither know, nor (by reason of the great distance betwixt our places of residence) have at present the opportunity to enquire, whether he will think fit to annex his Discourse to our *Appendix*, or to publish it by it self, or at all; and because he hath not yet, for ought I know, met with fit Glasses to make an any-thing accurate Table of the Decrement of the force of dilated Air; our present design invites us to present the Reader with that which follows, wherein I had the assistance of the same person that I took notice of in the former Chapter, as having writ-

ten something about Rarefaction: whom I the rather make mention of on this occasion, because when he first heard me speak of Mr. Townley's suppositions about the proportion where in Air loses of its Spring by Dilatation, he told me he had the year before (and not long after the publication of my *Pneumatical Treatise*) made Observations to the same purpose, which he acknowledged to agree well enough with Mr. Townley's Theory: And to did (as then Author was pleased to tell me) some Trials made about the same time by that Noble Virtuosa and eminent Mathematician the Lord Brouncker, from whose further Enquiries into this matter, if his occasions will allow him to make them, the Curious may well hope for something very accurate.

A Table of the Rarefaction of the Air

| A | B | C | D | E |
|-----------------|----|---|------------------|------------------|
| 1 | 00 | | 29 $\frac{1}{2}$ | 29 $\frac{1}{2}$ |
| 1 $\frac{1}{2}$ | 10 | | 19 $\frac{1}{2}$ | 19 $\frac{1}{2}$ |
| 2 | 15 | | 14 $\frac{1}{2}$ | 14 $\frac{1}{2}$ |
| 3 | 20 | | 9 $\frac{1}{2}$ | 9 $\frac{1}{2}$ |
| 4 | 22 | | 7 $\frac{1}{2}$ | 7 $\frac{1}{2}$ |
| 5 | 24 | | 5 $\frac{1}{2}$ | 5 $\frac{1}{2}$ |
| 6 | 24 | | 4 $\frac{1}{2}$ | 4 $\frac{1}{2}$ |
| 7 | 25 | | 4 $\frac{1}{2}$ | 4 $\frac{1}{2}$ |
| 8 | 26 | | 3 $\frac{1}{2}$ | 3 $\frac{1}{2}$ |
| 9 | 26 | | 3 $\frac{1}{2}$ | 3 $\frac{1}{2}$ |
| 10 | 26 | | 3 $\frac{1}{2}$ | 3 $\frac{1}{2}$ |
| 12 | 27 | | 2 $\frac{1}{2}$ | 2 $\frac{1}{2}$ |
| 14 | 27 | | 2 $\frac{1}{2}$ | 2 $\frac{1}{2}$ |
| 16 | 27 | | 2 $\frac{1}{2}$ | 2 $\frac{1}{2}$ |
| 18 | 27 | | 1 $\frac{1}{2}$ | 1 $\frac{1}{2}$ |
| 20 | 28 | | 1 $\frac{1}{2}$ | 1 $\frac{1}{2}$ |
| 24 | 28 | | 1 $\frac{1}{2}$ | 1 $\frac{1}{2}$ |
| 28 | 28 | | 1 $\frac{1}{2}$ | 1 $\frac{1}{2}$ |
| 32 | 28 | | 1 $\frac{1}{2}$ | 1 $\frac{1}{2}$ |

Subtracted from 29 $\frac{1}{2}$ leaves.

- A. The number of equal spaces at the top of the Tube, that contained the same parcel of Air.
 B. The height of the Mercurial Cylinder, that together with the Spring of the included Air counterbalanced the pressure of the Atmosphere.
 C. The pressure of the Atmosphere.
 D. The Complement of B to C, exhibiting the pressure sustained by the included Air.
 E. What that pressure should be according to the Hypothesis.

To make the Experiment of the debilitated force of expanded Air the plainer, 'twill not be amiss to note some particulars,

lars, especially touching the manner of making the Trial; which (for the reasons lately mention'd) we made on a light-some pair of Stairs, and with a Box also lin'd with Paper to receive the *Mercury* that might be spilt. And in regard it would require a vast and in few places procurable quantity of Quick-silver, to imploy vessels of such kind as are ordinary in the *Toricellian* Experiment, we made use of a Glas Tube of about six foot long, for that being Hermetically sealed at one end, serv'd our turn as well as if we could have made the Experiment in a Tub or Pond of seventy Inches deep.

Secondly, We also provided a slender Glas-Pipe of about the bigness of a Swans Quill, and open at both ends: All along which was pasted a narrow list of Paper divided into Inches and half quarters.

Thirdly, This slender Pipe being thrust down into the greater Tube almost fill'd with Quicksilver, the Glas helpt to make it swell to the top of the Tube, and the Quicksilver getting in at the lower orifice of the Pipe, fill'd it up till the *Mercury* included in that was near about a level with the surface of the surrounding *Mercury* in the Tube.

Fourthly, there being, as near as we could guess, little more than an Inch of the slender Pipe left above the surface of the restagnant *Mercury*, and consequently unfill'd therewith, the prominent orifice was carefully clos'd with sealing Wax melted; after which the Pipe was let alone for a while, that the Air dilated a little by the heat of the Wax, might upon refrigeration be reduc'd to its wonted density. And then we observ'd by the help of the above-mentioned list of Paper, whether we had not included somewhat more or somewhat less than an Inch of Air, and in either case we were sain to rectifie the error by a small hole made (with a heated Pin) in the Wax, and afterwards clos'd up again.

Fifthly, Having thus included a just Inch of Air, we lifted up the slender Pipe by degrees, till the Air was dilated to an Inch, an Inch and an half, two Inches, &c. and observed in Inches and Eighth, the length of the *Mercurial* Cylinder, which

which at each degree of the Air's expansion was impell'd above the surface of the restagnant *Mercury* in the Tube.

Sixthly, The Observations being ended, we presently made the *Torricellian* Experiment with the above-mention'd great Tube of six foot long, that we might know the height of the *Mercurial* Cylinder, for that particular day and hour; which height we found to be $29\frac{1}{2}$ Inches.

Seventhly, Our Observations made after this manner furnish'd us with the preceding Table, in which there would not probably have been found the difference here set down betwixt the force of the Air when expanded to double its former dimensions, and what that force should have been precisely according to the Theory, but that the included Inch of Air receiv'd some little accession during the Trial; which this newly-mention'd difference making us suspect, we found by replunging the Pipe into the Quicksilver, that the included Air had gain'd about half an eighth, which we guess to have come from some little *Aëreal* bubbles in the Quicksilver, contain'd in the Pipe (so easie is it in such nice Experiments to miss of exactness.) We try'd also with 12 Inches of Air shut up to be dilated; but being then hindered by some unwelcome avocations to prosecute those Experiments, we shall elsewhere, out of other Notes and Trials (God permitting) set down some other accurate Tables concerning this matter. By which possibly we may be assisted to resolve whether the *Atmosphere* should be look'd upon (as it usually is) as a limited and bounded Portion of the Air; or whether we should in a stricter sense than we did before, use the *Atmosphere* and *Aëreal* part of the World for almost equivalent terms; or else whether we should allow the word *Atmosphere* some other notion in relation to its Extent and Limits; (for as to its Spring and Weight, these Experiments do not question, but evince them.) But we are willing, as we said, to refer these matters to our *Appendix*, and till then to retain our wonted manner of speaking of the Air and *Atmosphere*. In the mean time (to return to our last-mention'd Experiments) besides that so little a variation may be in great
part

part imputed to the difficulty of making Experiments of this nature exactly, and perhaps a good part of it to something of inequality in the cavity of the Pipe, or even in the thickness of the Glafs; besides this, I say, the proportion betwixt the several pressures of the included Air undilated and expanded, especially when the Dilatation was great (for when the Air swell'd but to four times its first extent, the Mercurial Cylinder, though of near 23 Inches, differ'd not a quarter of an Inch from what it should have been according to Mathematical exactness) the proportion, I say, was suitable enough to what might be expected, to allow us to make this reflexion upon the whole, That whether or no the intimated Theory will hold exactly, (for about that, as I said above, I dare determine nothing resolutely till I have further considered the matter) yet *since* the Inch of Air when it was first included was shut up with no other pressure than that which it had from the weight of the incumbent Air, and was no more compress'd than the rest of the Air we breathed and moved in; and *since* also this Inch of Air, when expanded to twice its former dimensions, was able with the help of a Mercurial Cylinder of *about* 15 Inches to counterpoise the weight of the Atmosphere, which the weight of the external Air gravitating upon the restagnant *Mercury* was able to impell up into the Pipe, and sustain above twenty eight Inches of *Mercury* when the internal Air by its great expansion had its Spring too far debilitated to make any *considerable* (I say considerable, for it was not yet so dilated as not to make *some*) resistance: *Stnce*, I say, these things are so, the free Air here below appears to be almost as strongly compress'd by the weight of the incumbent Air as it would be by the Weight of a *Mercurial* Cylinder of twenty eight or thirty Inches; and consequently is not in such a state of laxity and freedom as men are wont to imagine; and acts like some mechanical Agent, the decrement of whose force holds a stricter proportion to its increase of dimension, than has been hitherto taken notice of.

I must not now stand to propose the several reflexions that may be made upon the foregoing Observations touching the Compression and Expansion of Air; partly because we could scarce avoid making the Historical part somewhat prolix; and partly because I suppose we have already said enough to shew what was intended, namely, that to solve the *Phænomena* there is not of our Adversaries *Hypothesis* any need: the evincing of which will appear to be of no small moment in our present Controversie, to him that considers, that the two main things that induced the Learned Examiner to reject our *Hypothesis* are, that Nature abhors a *Vacuum*, and that though the Air have some Weight and Spring, yet these are insufficient to make out the known *Phænomena*; for which we must therefore have recourse to his *Funiculus*. Now as we have formerly seen, that he has not so satisfactorily disproved as resolutely rejected a *Vacuum*, so we have now manifested that the Spring of the Air may suffice to perform greater things than what our Explication of the *Torricellian* Experiments and those of our Engine obliges us to ascribe to it. Wherefore since besides the several difficulties that incumber the *Hypothesis* we oppose, and especially its being scarce, if at all, intelligible, we can add that it is *unnecessary*; we dare expect that such Readers as are not byas'd by their reverence for *Aristotle* or the *Peripatetic* Schools, will hardly reject an *Hypothesis* which, besides that it is very *intelligible*, is now prov'd to be *sufficient*, only to imbrace a Doctrine that supposes such a rarefaction and condensation, as many famous Naturalists rejected for its not being comprehensible, even when they knew of no other way (that was probable) of solving the *Phænomena* wont to be explicated by it.

The III. Part.

Wherein what is objected against Mr. Boyle's Explications of particular Experiments, is answered.

AND now we are come to the third and last Part of our Defence; wherein we are to consider what our Examiner is pleas'd to object against some passages of our *Physico-Mechanical Treatise*. But though this may seem the only part wherein I am particularly concern'd; yet perhaps we shall find it, if not the shortest, at least the easiest, part of our Task. *Partly*, because our Author takes no exceptions at the Experiments themselves, as we have recorded them (which from an Adversary, who in some places speaks of them as an Eye-witness, is no contemptible testimony that the matters of fact have been rightly delivered:) and *partly*, because there are divers Experiments which, together with their Explications, the Examiner has thought fit to leave untoucht, and thereby allows us to do so too: and *partly* also, because that (as to divers of those Experiments upon which he animadverts) he does not pretend to shew that our Explications are ill deduc'd or incongruous to our Principles; but only that the *Phænomena* may be explain'd either better or as well by his *Hypothesis*, whereof he supposes himself to have demonstrated the truth, together with the erroneousness of ours, in the other parts of his Book, especially the third, fourth and fifth Chapters. So that after what we have said to vindicate the *Hypothesis* we maintain, and take away our Author's imaginary *Funiculus*; it will not be requisite for us on such occasions to examine his particular Assertions and Explications. Which Advertisement we hope the Reader will be pleas'd to bear in mind, and thereby save himself and us the trouble of a great deal of unnecessary repetition. Wherefore presuming he will do so, we shall not stay to examine the first and second Corollaries, which in his

17 Chapter he annexes to the manner of emptying our Receiver by our Pump. Neither should we say any thing as to his third and last Corollary, but that we think fit to desire the Reader to take notice, that according to what he teaches in that place, the more the Air is rarefied, the more forcibly it is able to contract it self.

A defence of our 1. and 2. Experiments.

And to proceed now to his 18 Chapter, which he intitles *De Experimentis Boylianis*, we shall find according to what we lately noted, that against the first Experiment he objects nothing save that, if one of the Fingers be applied to the Orifice of the Valve when the Pump is freed from Air, the Experimenter shall feel to his pain that the Sucker is not thrust inward by the external Air, but, as the Finger, drawn inward by the internal. But this *Phenomenon* of the intrusion of the Finger into a Cavity, where it finds no resistance; having been formerly accounted for according to our *Hypothesis*, we shall not need to repeat our Explication of it; though this mistaken *Phenomenon* supplies our Adversary with divers of his following Animadversions, and indeed with a great part of his Book. And accordingly his Objection against our Second Experiment being of the same nature with that against the first, requires but the same Answer: For it will not alter the case that he adds upon this Experiment, *Hoc esse discrimen manifestum inter pressionem & suctionem, quod suckio efficiat hujusmodi adhesionem, pressio autem minime*; since to say so is but to affirm, not to prove.

The 3. Experiment.

What our Author would except against the 3. Experiment he ought to have more intelligibly exprest: For whereas of a Discourse wherein I deliver several particulars, he only says that *Nullatenus satisfacit, ut legenti constabit*; I would not do the Reader the injury to suspect him of taking this proofless Assertion for a rational Confutation; especially since upon the re-
view

view of that third Experiment I find nothing that agrees not with my *Hypothesis*, however it may disagree with the Examiners. But, to consider the Explication he substitutes in the room of our Doctrine, which he rejects, he gives it us in these words; *Hoc quoque Experimentum principiis nostris optime convenit: Cum enim per illam Emboli depressionem aer in cavitate brachii inclusus separetur ab eodem brachio, descendatque simul cum Embolo (uti de aqua simul cum argento vivo descendente capite decimo tertio vidimus) sit ut in tota illa depressione, novæ semper ab aëre illo descendente superficies deripiantur simul & extendantur, ut ibidem de aqua est explicatum: cum itaque æque facile diripiantur & extendantur hujusmodi superficies in fine depressionis ac initio, mirum non est quod eadem utrobique sentiatur deprimenti difficultas.*

By which though he seems to intend an Opposition to that part of the third Experiment which I oppos'd not against his Opinion, but that of some learned Vacuists: Yet (not to mention that he seems to have somewhat mistaken my sense) he offers nothing at all to invalidate my inference against them; but instead of that proposes a defence of his own Opinion, which supposes the truth of his disproved *Hypothesis*, and is either unsatisfactory even according to that, or else disagrees with what himself hath taught us but a little before. For 'tis evident that the more the Sucker is depress'd, the more the Cylinder is exhausted of Air. And in his third Corollary (which we lately desired the Reader to observe) speaking of the Air in the Receiver (and the case is the same with the Air in the Cylinder) he affirms more than once, *Eo magis extendi ac rarefieri aërem reliquum, quo plus inde exhauritur, majoremque proinde acquirere vim sese contrahendi.* Whereas here he would have us believe, that the little internal Air that was in the Cavity of the Shank of the Stop-cock, does as strongly retract the Sucker, or, which in our case is all one, resist its depression, when the Sucker is yet near the top of the Cylinder, (and consequently when the included Air is but a little dilated) as when the same Sucker being forced down to the lower part of the

Cylinder, the same portion of remaining Air must be exceedingly more distended.

The 4. Experiment.

In the Fourth Experiment, touching the swelling of a Bladder upon the removal of the ambient Air; and proportionably to that removal: Our Author objects nothing against the Explication we give of it by the Spring of the Air included in the Bladder, and distending it according as the pressure of the ambient Air is weakned. But he endeavours also to explicate it his way, to which he says this circumstance does excellently agree, that upon the regress of the external Air into the Receiver, the tumid Bladder immediately shrinks, because (saith he) by such ingress of the external Air, the Air in the Receiver, which drew the sides of the Bladder outward from the middle of it, is relax'd. Which Explication whether it be more natural than ours (that ascribes the shrinking of the Bladder to the pressure of the Air that is let into the Receiver) let the Reader judge, who has considered what we have formerly objected against the Examiners *Funiculus*, and the Relaxation of it upon the admission of Air.

As for the reason likewise he adds, why a perforated Bladder does not also swell, namely, that by the hole, how little soever, the included Air is suck'd out by the rarefied ambient, we leave it to the impartial Reader to consider whether is the more genuine Explication, either ours (against which he has nothing to object) or his, which to make clearly out he ought (according to what we formerly noted disputing against his *Funiculus*) to shew us what kind of strings they are; which though, according to him, strongly fastned to the inside of the Receiver and the *superficies* of the Bladder, must draw just as forcibly one as another, how long soever they be without the Bladder in comparison of those that within the Bladder draw so as to hinder the diduction of its sides. For Experience shews, that in a perforated Bladder the wrinkles continue as if there were no drawing at all.

And

And though he could describe how such a string may be context, yet our Explication will have this advantage in point of probability above his, That whereas he denies not that the Air has Spring and Weight, as we deny his *Funiculus* to have any other than an imaginary existence; and whereas he acknowledges that by the Instrument the Air about the Bladder is exhausted; to shew that there needs no more than that, and consequently no *Funiculus*, to draw asunder the sides of the Bladder, we can confirm our Explication by the formerly-mentioned Experiment of the ingenious *Paschal*, who carrying a flaccid Foot-ball from the bottom to the top of an high Mountain, found it to swell proportionably as he ascended, and as the weight and pressure of the ambient Air decreased, and likewise to shrink again as he descended. And yet in this case there is no recourse to be had to a *Funiculus* of violently-rarefied Air to draw asunder every way the sides of the Foot-ball. But however the Examiner will be able to defend his Explication, it may suffice us that he has objected nothing against ours.

The 5. Experiment.

Against the cause we assign of the fifth Experiment he likewise objects nothing, but only ascribes the breaking of the Bladder to the self contraction of the rarefied Air in the Receiver. And therefore referring the Reader to what we have newly said about the last Experiment, we will with our Author pass over the sixth and seventh, to which he has no quarrel, and proceed to the eighth.

The 8. Experiment.

This is that wherein we mention our having broke a Glass-Receiver, which was not globular, by the exhaustion of most of the inward Air, whereby its debilitated pressure became unable to resist the unweakened pressure of the outward Air. But this Explication the Examiner confidently rejects in these words, *At prope non videtur credibile, molliſſimum hunc æræom tam vehementer*

hementer vitrum (tanta præsertim crassitudinis quanta ibidem dicitur) undiq; sic comprimere ut illud perfringat: as if it were more credible that the little Air within (which, according to him, is so much thinner than common Air) should be able to act more powerfully upon the Glass than the Air without, which himself confesses to be a heavy body, and which not only reaches from the surface of the Earth to the top of the highest Mountains, but which (as may not improbably be argued from what we have elsewhere delivered) may, for ought we know to the contrary, be heaped upon the Receiver to the height of some hundreds of Miles, nay, to I know not how many thousands, in case the Atmosphere be not a bounded portion of the Air, but reach as high as It.

As for the Explication he substitutes in these words, *Verum itaq; respondetur, ideo sic fractum esse illud vitrum, quia per exhaustionem illam latera ejus vehementius introrsum sunt attrita, quam ut (ob figuram illam resistendo minus idoneam) resistere potuerint. Cum enim inclusus aer lateribus vitri firmissime adhaereat, nihil aliud erit aërem illum sic exhaurire, quam satagere latera vitri introrsum flectere:* By what we have already discoursed about the *Funiculus*, the Reader may easily discern what is to be answered. Nor does our Author here shew us any way by which his imaginary strings should take such fast hold of the sides of the Glass, as to be able to draw them together notwithstanding the resistance they find from the close texture of the Body to be broken.

The 9. Experiment.

Our Explication of the ninth Experiment he handles very severely: for having briefly recited it, he proposes his Objection against it thus, *Sed profecto nimis longe videtur hoc à veritate recedere: potestq; vel inde solum satis refutari; quia si tanta sit pressura aëris sic per tubum illum in phialam descendantis, ut ipsam phialam perfringat, deberet profecto inclusam aquam, cui im-*

mergitur illa tubus, valde quoque ante fractionem phiala commovere, bullulasque in eadem excitare, &c. ut constat, siquis, insufflando per illum tubulum, aquam vel mediocriter sic premat. At certum est aquam, antequam frangatur sic phiala, nec tantillum moveri: ut experienti constabit.

But, I do confess, I do for all this think our Explication more true, than well considered by our Author. For the putting of water into the Vial that was broken, was done (as is clearly intimated in the beginning of our Narrative) upon a particular design (as indeed we tryed divers other things with our Engine, not so much with immediate reference to the Spring of the Air, as to make use of such Tryals in some other of our Writings.) And accordingly in the second Tryal mentioned in the same Experiment the water was omitted. But, notwithstanding this water, the sides of the Glas being expos'd to the pressure of the Atmosphere, had that whole pressure against them before the exhaustion of the Receiver; so that there needed no such blowing in of the Air afresh as our Author imagines, to effect the breaking of the Vial, it being sufficient for that purpose, that the pressure against the convex superficies of it was taken off by the exhaustion of the Receiver, the pressure against the concave superficies remaining as great as ever. And therefore we need not altogether deny what the Examiner says that *Licet clausus superne fuisset tubulus ille, eodem tamen modo fracta sine dubio fuisset phiala.* For, since in such cases the Air (as we have often taught) is shut up with the whole pressure of the Atmosphere upon it, it may almost as easily break the Glas as if it were unstopt. And accordingly we mention in the 36. Experiment the breaking of a thin Glas Hermetically seal'd upon the recess of the ambient Air. But, how confidently soever our Author speaks, I thought fit to adde the word *almost*, because we observed in the 39 Experiment, that such thin Vials (and thick ones will not break) are subject upon the withdrawing of the ambient Air to retch a little, whereby the Spring of the Air within the Vial might in some cases (I say, in some) be so far weakned as not to be able to break it, unless

assisted by the pressure of the Atmosphere wherewith it communicates, and which leans upon it. And when the Vial does actually begin to break, then the pursuing pressure of the outward Air upon the yielding Air within the Vial may help to throw the parts of the Glas more forcibly asunder.

All the Experiments from the 9. to the 17. exclusively our Examiner leaving uncensured, we may with him advance to the consideration of the 17.

The 17 Experiment defended.

In this we relate how, when we made the *Torricellian* Experiment, we shut up the restagnant *Mercury* together with the Tube and the suspended Mercurial Cylinder (of about 29 Inches) in our Receiver, that by drawing off and letting in the Air at pleasure upon the restagnant *Mercury*, and consequently weakning and increasing its pressure, we might make it more clearly appear than hitherto had been done by Experiment, that the suspension of the Mercurial Cylinder, and the Height of it, depended upon the greater or lesser pressure of the Air. But against our Explication of this Experiment (which has had the good fortune to convince and satisfie many ingenious men) the Examiner objects nothing in particular, contenting himself to have recourse here also to his *Funiculus*. Yet two observations of ours he is pleased to take notice of.

The first is, that though the Quicksilver were exactly shut up into our Receiver after the manner newly declared, yet the suspended Quicksilver did not descend: whence having said that I argue, that it is now sustained not by the Counterpoise of the Atmosphere, but by the Spring of the Air shut up in the Receiver, he subjoyns onely this, *Sed rectius sane inferitur, Cylindrum illum nihil ibidem antea pressuisse*. But whether this be not *gratis dictum* we leave the Reader to collect from what we have formerly discoursed in the second Part of this Defence of the Spring of the Air; especially from that Experiment; by which it appears, that Spring may sustain a far higher Cylinder of Quicksilver.

In the second Observation he mentions of ours, he summarily recites our Explication of the descent and ascent of the *Mercury* in the Tube, by the debilitated and strengthened Spring of the Air. But without finding fault with our application of that principle to the *Phænomena*, he says that he has sufficiently refuted the principle it self in the fourth Chapter, (which how well he has done we have already seen) and therefore explicates the matter thus; *Dico igitur* (says he) *argentum per illam exhaustiōnem sic in tubo descendere, quod deorsum trahatur ab aëre qui incumbit argento restagnanti: siquidem incumbens ille aër jam per exhaustiōnem valde rarefactus & extensus, sese vehementer contrahit, & contrahendo conatur etiam subiectum sibi argentum restagnans è suo vasculo elevare, unde fit ut (argento illo restagnante minus jam gravitante in fundum sui vasculi) argentum quod est in tubo descendat; ut per se patet. Adeoq; mirum non est, quod, ingrediente postea aëre externo, rursus argentum ascendat, cum per illum ingressum vis illa sic elevans argentum restagnans debilitetur.* But this Explication supposing such a *Funiculus* as we have already shewn to be but fictitious, the Reader will easily gather what is to be judged of it from what has been already delivered. Wherefore I shall onely subjoyn, that by this Explication, were it admitted, there is onely an account given of that part of our seventeenth Experiment which relates to the descent of the *Mercury* below its wonted height, and its re-ascent to it. But as for our having by the forcing in some more Air into the Receiver, impell'd the Quicksilver to a considerably-greater height than 'tis wont to be sustain'd at in the *Torricellian* Experiment, I confess I understand not how the Examiner gives an account of it in the following words, (which are immediately annex'd to those we last recited of his, and which are all that he employes to explicate this notable *Phænomenon*) *Atq; hinc etiam redditur ratio alterius quod ibidem quoq; notatur, nempe quod per violentam intrusionem aëris externi in Recipientem, ascenderit argentum notabiliter supra digitos 29.* Nam sicut per extractionem aëris argentum infra stationem detrahatur, sic etiam per intrusionem

novi supra eandem elevabitur. For in this passage I see not how he himself does not rather repeat the matter of fact, than give any account how it is perform'd. And if it be alledged on his behalf, That according to his principles it may be said that, upon the pressure of adventitious Air upon the restagnant *Mercury*, the *Funiculus* in the Tube, that was not able before to draw it up above 29½ Inches, is now enabled to draw it up higher; I demand upon what account this new Air does thus press against the restagnant *Mercury*, and impell up and sustain that in the Tube. It will not be said that 'tis by its weight; for as much *Mercury* as may be thus impell'd up above the usual station will weigh a great many times more than the Air forc'd into the Receiver. And therefore it remains that the additional Air counterpoises the additional *Mercury* by its Spring. And if we consider withall, that there's no reason to doubt, (especially considering what we have formerly delivered upon tryal touching the power of compress'd Air to impell up Quicksilver) but that, had we not been afraid of breaking our Vessel we might by forcing more Air into the Receiver have impell'd it up to the top of the Tube, and kept it there; we shall scarce deny but that, supposing there could be no such *Funiculus* as our Examiner's *in rerum natura*, the pressure of the incumbent Air alone might suffice to keep a correspondent Cylinder of *Mercury* suspended: and that without any attraction of the restagnant *Mercury* by a *Funiculus* of violently-distended Air in the Receiver, the Quicksilver in the Tube may be made to rest at any height greater or lesser, provided it exceed 30 Inches, onely because its weight is just able to counterbalance the pressure of the contiguous Air.

I know not whether I may not adde (to express an unwillingness to omit what some may think proper to do my Adversary right) that it may be said for the Examiner, that he in the 11 page acknowledging with us a power in the Air to recover its due extension if it be croud'd into less room than its disposition requires; a man may from that principle solve the *Phænomena* in question by saying, that the Air in the Receiver being

being forcibly compress'd by the intrusion of fresh Air into the same vessel, does by its endeavour to recover its due expansion press upon the restagnant *Mercury*, and force up some of it into the Tube. But this Explication, though it agree with what the Author teaches in a place very distant from his Notes upon our 17 Experiment, now under debate; yet still 'tis not clear to me how, by what he sayes in these Notes, the *Phænomenon* is accounted for as the word *Hinc* imports it to be. But otherwise I need not quarrel with the Explication, since without recurring to the *Funiculus* for the sustaining of the additional *Mercury*, the solution of the *Phænomenon* is given upon the same principle that I employ.

The 18 Experiment.

Our Examiner in his Animadversion upon the 18 Experiment, having recited my Conjecture as the cause why a Cylinder of *Mercury* did in Winter rise and fall in the Tube, sometimes as Water is wont to do in a Weather-glass, according to the laws of heat and cold, and sometimes quite contrary thereunto; adds, that this Experiment does strongly enough overthrow our *Hypothesis* of the Atmospherical Cylinder, and clearly shew that the Quicksilver is not sustained by it: *Nam* (sayes he) *si hic ab eo sustentatum fuisset, debuisset potius frigidore tempore ascendere quam descendere, eo quod aër tunc multo densior esset & gravior. Itaq; non sustentatur argentum ab aëris equipondio, ut asseritur.* And by the same Argument he concludes against the *Mercury's* being sustained by the Spring of the Air. But in his Animadversions upon this Experiment he seems to have been too forward to reprehend; for he neither well confutes my Conjecture, nor substitutes so much as a plausible one in the stead of it. And as to his Objection I answer,

First, That it doth not conclude: because that as sometimes the Quicksilver in the Tube did rise in warmer, and fall in colder, weather; so at other times it did rather emulate the ascent and descent of water in a Weather-glass.

Secondly, Though it be true, that Gold is wont to condense this or that parcel of Air, and that a parcel of Air may be made heavier by Condensation; yet that is in regard of the ambient Air that retains its wonted laxity, in which the condensed Air is weighed. But our Author has not yet proved, that in case the cold of the Winter should condense the whole incumbent Atmosphere, it would then gravitate sensibly more upon the restagnant Quicksilver than before. As a Pound of Wooll will not sensibly vary its weight, though the hairs whereof it is composed be made to lie sometimes in a looser, sometimes in a closer, order.

And, thirdly, this Objection does as little agree with his Doctrine as with my Conjecture: For in the 50. page, where he gives us an account according to his principles of the rising and falling of water in a Weather-glass, and compares it with the suspension of Quicksilver, he tells us, *Hinc fit quod, contracto hoc funiculo per frigus, aqua illa tempore frigido ascendat, descendat autem tempore calido, eo quod per calorem funiculus ille dilatetur.* So that, according to the Examiner himself, the Quicksilver ought to have ascended in colder, and descended in warmer, weather. Now, although I proposed my thoughts of the difficult *Phenomenon* under consideration but as a Conjecture, and therefore shall be ready to alter them, either upon further discovery, or better information; yet I see not why it should be post-posed to the Examiner's, who, though he rejects our Explication, substitutes no other than what may be gathered from these words, *Ego certe non dubito quin dentur huiusmodi occultæ causæ, quibus funiculus ille subtilis quo in tubo suspenditur argentum (ut dictum est capite decimo) modo producat, modo abbrevietur, &c. sicq; argentum nunc demittat, nunc elevet.* For, since we have made it probable that the copious Fumes sometimes suddenly ascending into the Air, and rolling up and down in it, sometimes sensibly altering (if good Authors may be credited) the refraction of it, and since some other causes, mentioned in our eighteenth Experiment, may alter the density and gravity of the Air that leans upon the restagnant Mercury; I suppose

suppose the Reader will think it more intelligible, and probable that alterations, other than those produced by heat and cold may happen to the incumbent Atmosphere, which freely communicates with the neighbouring Air, and may thereby become sometimes more stuffy, and sometimes more destitute of adventitious Exhalations; than that such changes should happen to a *Funiculus* included in Glass, which according to our Author is impervious to the subtlest steams that are, and concerning which he offers not so much as a Conjecture upon what other account it can happen to be sometimes contracted, and sometimes stretch'd.

The 19 Experiment.

Upon this the Examiner has onely this short Animadversion, *In decimo nono ostendit aquam eodem modo per exhaustiōnem Recipientis descendere, quo in precedente descendere ostendat argentum vivum; cujus cum eadem sit ratio, non est cur amplius ei insistantur.* In which words since he offers nothing new or peculiar to shew any incongruity in our Explication to our principles, which agree very well with the new *Phænomena* of the Experiment; we are content to leave the Reader to judge of the *Hypotheses* themselves, which of the two is the more probable, either ours, that onely requires that the Air in the Receiver should equally resist a Cylinder of Water and of Quicksilver, when their weight is but the same, though their altitudes be not; or the Examiner's, which exacts that (according to what we formerly elsewhere noted) Bodies of such differing nature and texture as Quicksilver and Water should need but just the same weight or strength to rarefie them into a *Funiculus*.

The 20 Experiment.

In his *Examen* of this Experiment our Author makes me infer from the *Phænomena* he repeats, that not onely the Air, but the Water also has a Spring. But though I suspect not that he does wilfully mistake my sense, yet by what I write in this and the following Experiments the Reader may well enough perceive,

perceive, that I spoke but very doubtfully of a Spring in the water; nay, and that I did in the 154 page expressly teach, That the intumescence of it might (at least in great part) proceed from that of the small parcels of Air, which I thought to be usually harboured in the body of that liquor.

But whereas I ascribe the appearance of the Bubbles in the water to this, that upon the exhaustion of some of the Air incumbent on the water, the pressure of what remains is much debilitated, whereby the little Particles of Air lurking in the

Water are allowed to expand themselves into bubbles; *Sed contra* he rejects this Explication as manifestly false: *manifeste.* Nam

(sayes he) *si ita fieret, deberent profecto hujusmodi bullulae non è fundo vasis sic ascendere, (uti tam in hoc quam in sequentibus experimentis in quibus de istis bullis agitur semper asseritur) sed è superiore parte aquae, ubi minus premuntur, ut per se est manifestum.* But why he should be here so peremptory I confess I do not, for all this Objection, yet see: For in the bottom of the next page he sayes, he will not deny but that Aërial Particles latitant in the other parts of the water (he had before spoken of the bottom of it) may be extended into bubbles by his way of Rarefaction. And that we particularly mentioned the rising of bubbles, even from the bottom of the water, was because that circumstance seem'd to deserve a peculiar note; and not (as he seems to imagine) as if the bubbles did not also rise from the superior parts of the liquor, since we did take notice of it about the middle of the

See also in the 43 Experiment. these passages,—And this Effervescence was so great in the upper part of the water, &c. As also,—The Effervescence was confin'd to the upper part of the water, unless, &c.

149 page. And we often in this and the following Experiments observ'd, that the ascending bubbles grew bigger the nearer they came to the top. Which agrees more clearly with our Hypothesis wherein their conspicuous swelling as they ascend is attributed more to the lessening of the pressure of the incumbent Air than to the decrement of the weight of the incumbent water, (since when the surface of this liquor is lean'd upon by the Atmosphere, the ascending bubbles scarce sensibly increase

in Vessels no deeper than ours) then with the Explication which the Examiner gives in these words, *Respondeo, aquam per illam aeris exhaustionem non sponte sic ascendere, sed sursum violenter trahi, ac elevari à rarefacto illo aëre sese contrahente. Quemadmodum enim aqua aliqualem patitur compressionem (ut experientia constat) ita & aliqualem quoq; hic patitur distentionem. Atq; hinc clarè patet, cur potius à fundo vasis quam à parte aquæ superiore oriantur hujusmodi bullæ. Cum enim vehemens illa suctio co- netur aquam à fundo phialæ elevare, nascitur ibidem subtilis quædam materia quæ in bullas conversa sic ascendit, uti capite decimo quinto in quarto Experimento dictum est.* For, whatever he may think, it does not hence so clearly appear how the endeavour only of the *Funiculus* to draw up the Water from the bottom of the Vial, to which, that endeavour notwithstanding, it remains contiguous, should generate in some parts of the bottom of the Glass, and not in others, such a subtile matter as he tells us of. And I suppose the Reader will, as well as I, wish he had more intelligibly declared how this strange generation of subtile matter comes to be effected. And I presume it will likewise be expected that he also declare, why both in our case and in the *Torricellian* Experiment the bubbles grow so much larger by being nearer the top of the liquor; if, as he rejects our Explication of this Circumstance, the effect of the suction he speaks of be greater upon the lower part of the liquor than the upper, to which alone nevertheless his *Funiculus*; that is said so to draw the liquor, is contiguous.

Our Author making no particular Objection against the 10 following Experiments, we also shall pass them by, and fall with him upon the consideration of the 31 Experiment.

The 31 Experiment.

Upon this our Author having recited our Conjecture as the cause why two very flat and smooth Marbles stick so closely together, that by lifting up the uppermost you may take up also the lowermost, approves my way of examining that Conjecture. But whereas I say that the reason why, though the Mar-

bles were kept together by the pressure of the ambient Air, yet they did not fall asunder in our exhausted Receiver, no not though a weight of 4 Ounces were hung at the lower stone, might be, that by reason of some small leak in the Receiver the Air could not be sufficiently drawn out: yet he tells us with his wonted confidence, *Certum esse, sententiam illam vel hoc solo Experimento satis refelli*. But possibly he would have spoken less resolutely, if he had made all the trials about the adhesion of Marbles that we relate our selves to have made in the short History we have publish'd of *Fluidity and Firmness*. For our Examiner speaks as if all that we ascribe to the Air in such Experiments were to sustain the lower Marble with the weight perhaps of a few Ounces: Whereas in case the Air be kept from getting in at all between the stones, it may (according to our *Hypothesis*) sustain a Weight either altogether or well-nigh equal to that of a pillar of Air as broad as the Basis of the lower Marble, and as long as the Atmosphere is high, or to the weight of a pillar of Quicksilver of the same thickness, and about 30 Inches long; these two pillars appearing by the *Toricellian* Experiment to counterpoise each other. And therefore since in the seventeenth Experiment, when we had exhausted our Receiver as far as we could, there remain'd Air enough to keep up in the Tube a Cylinder of about an Inch long of Quick-silver; and since the broader the contiguous Marbles are, the greater weight fastned to the lowermost may be sustain'd by the resistance of the Air, (as is obvious to him that considers the *Hypothesis*, and as we have proved by Experiment in the forementioned *Tract*) it need be no wonder that the Air remaining in the Receiver should be able to support the lowermost Marble, whose Diameter was near two Inches, and a weight of four Ounces, those two Weights being inferior to that of a Mercurial Cylinder of that Diameter and an Inch in length. And though it were not, yet we are not sure that the Receiver was as well emptied when we made the 31 Experiment, as when we made the 17. And (if my Memory does not much mis-inform me) 'twas with the same pair of Marbles that in the

p. 9

presence.

presence of an illustrious Assembly of *Virtuosi* (who were Spectators of the Experiment) the uppermost Marble drew up the lowermost, though that were clogg'd with a weight of above 430 Ounces.

As for the account the Examiner substitutes of our *Phænomenon*, I know not whether many Readers will acquiesce in it: For, not to insist upon the Objection which himself takes notice of, that according to him the distended Air in the Receiver should draw asunder the adhering Marbles; his Explication supposes that there cannot naturally be a *Vacuum*, whence he infers that, *Neceſſe erat ut lapis ille non aliter descenderet, quàm relinquendo post se tenuem hujusmodi substantiam, qualis ab argento vivo aut aquâ sic descendentibus relinqui solet.* But whereas he adds, that the cause of the obstinate adhesion we meet with in our case is, that such a substance is far more difficult to be separated from Marble than from Quicksilver or any other kind of Body; that Assertion is precarious. And though I have tried Experiments of this nature with stones of several sizes, perhaps an hundred times, yet I never could find that by their cohesion they would sustain a weight greater than that of a Pillar of the Atmosphere that prest against the lowermost: Which is a considerable Circumstance, that much better agrees with our Explication than our Adversaries. And whereas he further says, *Unde existimo planè, si perfectè complanata fuerint duo marmora sic conjuncta, ita ut nullus omnino aër inter utrumq; mediet, non posse ea ullis humanis viribus ab invicem divelli:* I hope I need not tell the Reader, that whether or no this agree with what he had immediately before taught of the separableness of a subtile substance even from Marble, so bold and improbable an Assertion requires the being countenanc'd with a much better proof than the only one he subjoyns in these words, *Ulti etiam confirmat exemplum quod ibidem adducit Author de lamina enea, tabula cuicdam marmorea ita adherente, ut à lacertosa juvenè, de suis viribus gloriante, non potuerit per anulum centro ejus affixum inde elevari.* For sure there is great odds betwixt the strength of a man unassisted by any En-

gine, and the utmost extent of Humane Power. And indeed according to our *Hypothesis*, and without having recourse to Natures dreading of a *Vacuum*, the case is clear enough: For, supposing the Plate to be of any considerable breadth, the Pillar of the Atmosphere that lean'd upon it, and must at the instant of its deserting the *superficies* of the Table all at once be lifted up with it, may well exceed the force of a single man, especially in an inconvenient posture; since by the cohesion of a pair of Marbles of about three Inches Diameter, I did with my own hands take up above a thousand and three hundred Ounces.

The 32 and 33 Experiments.

Against our Explication of these two, which our Author examines together, he objects nothing peculiar, but contents himself to explicate them by his *Funiculus*: Wherefore neither shall we need to frame any peculiar defence for it, especially if the Reader will be pleased to refer hither as much of what we oppos'd to his Animadversion on the third Experiment as is justly applicable to our present Controversie. Our Author indeed endeavours to prove his Explication by saying, that the distended Air in the exhausted Cylinder draws up the Sucker with the annexed weight, *Eodem fere modo quo videmus in cucurbitulis dorso ægrotantis applicatis, in quibus, extincta jam flamma, rarefactus aer se contrahens carnem tam vehementer, uti videmus, elevat attrahitq; intra cucurbitulam.* But that Phenomenon is easily enough explicable in our *Hypothesis*, by saying, that upon the vanishing of that heat which strengthened the pressure of the included Air, the Spring of it grows too weak to resist any longer the pressure of the ambient Air; which thereupon thrusts the flesh and neighbouring blood of the Patient into the Cupping-glass, almost after the same manner as we formerly taught the Pulp of the Finger to be thrust into the deserted Cavity of the Glass-Tube in the *Torricellian Experiment*.

The 34, 35 and 36 Experiments.

To these our Author saying nothing but this, *In his tribus nihil peculiariter occurrit hic explicandum, cujus ratio ex jam dictis non facile pateat*; we also may be allow'd to pretermitt them, and pass on to

The 37 Experiment.

Of the appearance of Light or Whiteness, mentioned in this Experiment, the Examiner confesses that we have assigned a cause probable enough, by referring it to the vehement and sudden commotion of the included Air. And indeed though I do still look upon some of the things that I hesitantly propos'd about this difficult *Phænomenon* but as mere Conjectures, and though he annexes his Explication of it; yet I see not but that it is coincident with ours, or not better than it. For, to what I had said of the Commotion of the parts of the Air, he adds only in two or three several places their being violently distended; which how it improves the Explication of the *Phænomenon* I do not readily see. And whereas he subjoyns, *Existimo autem dicendum potius candorem illum esse lumen quoddam reflexum, quam innatum, eo quod (ut testatur Author) in tenebris non appareat, sed solum de die aut accensa candela*: I presume the attentive Reader will easily discern that his Opinion is much what the same that I propos'd and grounded on the same reason. But the chief difficulty in this abstruse *Phænomenon*, namely why we meet with it but sometimes, our Examiner's Explication leaves untouch'd.

The 38 and 39 Experiments.

Against these our Author makes no peculiar Objections.

The 40 and 41 Experiments.

But in his Animadversions upon these, having told the Reader that I seem to ascribe the sudden extinction of the included Animals to the excessive thinness of the Air remaining,

in the Receiver, made by the recess of what was drawn out, unfit for Respiration; he adds resolutely enough, *Verum impossibile videtur, ut hujusmodi animalcula ob solum defectum crassioris aëris tam cito moriantur*: But gives no other reason than that they dye so soon, which is no more than what he said in the newly-cited words, and besides is grounded upon something of mistake. For the Creatures he mentions were a Bee, a Fly, and a Caterpillar, and those included too in a small Receiver, which could be suddenly exhausted: and these indeed became moveless within a minute of an hour; but that minute was not (as the word is often us'd to signify in *English*) a Moment, but the Sixtieth part of an Hour. And though these Insects did in so short a time grow moveless, yet they were not so soon kill'd; as appears by the Narrative. The sanguineous Animals that did *indeed* dye, were kill'd more slowly. And I remember that having purposely enquir'd of a man (us'd to go under water by the help of an Engine wherein he could carry Air with him to the bottom of the Sea) how long

See more concerning this Objection in the Answer to it as 'tis propos'd by Mr. Hobbs.

he could endure, before he was accustomed to dive, without breathing or the use of a Sponge; he told me, that at first he could hold out about two or three Minutes at a time: Which made me think that Divers become able to continue under Water so long, either by a peculiarly-convenient Constitution of body, or by a gradual exercise. And I am apt to think that he did, as men are wont to do, when he said two or three Minutes, mean what is indeed a much shorter time than that when exactly measured amounts to. For, having purposely made trial upon a couple of Moles that were brought me together alive, one of them included in a small, though not very small, Receiver was between two and three Minutes in killing; whereas the other being immediately after detain'd under Water did not there continue full a Minute and a quarter, before it finally ceas'd from giving any sign at all of life. By which trial it may appear, that 'tis not impossible that the want of Respiration should dispatch an Animal in a little

time

time as is mentioned in the Experiment I am now defending. And indeed our Author either should have proved that 'tis not possible for the want of Air to destroy Animals so soon, or should have given us some better account of the *Phænomenon*. For whereas he teaches us, that according to his Doctrine the little Animals above mentioned were so soon kill'd, *quæper rarefactum illum aërem sese contrahentem extraxerunt sit eorum halitus*: I see not that hereby, if he explicate the *Phænomenon* otherwise than we, he explains it better; for he seems to speak as if he thought this *halitus* to be some peculiar part of the Animal in which his life resides. And besides he seems not to consider, that whereas, according to me as well as according to him, the Air contained in the Lungs (supposing these *Animalcula* have any) must in great part pass thence into the Receiver, (for whether that be done by the Spring of the Air it self, that was harboured in the Lungs, or the traction of the more rarefied Air in the Receiver, is not material in our present case) the Examiner must, as well as I, render a reason why the extenuation or recess of the *halitus* should cause the hasty death of the included Animals; and condemning my Conjecture he ought to have substituted another reason: and though he subjoyns these words, and concludes with them, *Atq; hinc quoq; ortæ sunt vehementes illæ convulsiones, quas ante mortem passas esse aviculas quasdam memorat ibidem Author*; yet I doubt not but the Reader will think it had not been amiss that the Author had more intelligibly reduc'd these Tragick Symptoms from his Assumption, for the sake of those that are not Anatomists and Physicians enough to discern how his *Funiculus* could produce these effects.

For my part, as in the 41 Experiment I tender'd my thoughts concerning Respiration but doubtingly, so I am yet unwilling to determine resolutely in a matter of that difficulty.

The 42 and 43 Experiments.

In his *Examen* of these two last of our Physico-Mechanical Experiments, the Author contents himself to endeavour to explicate

plicate the *Phænomena* recited in them by the contraction of the raref'd Air; which, according to him, endeavours to draw up the subjacent water out of the Vial, whereby it vehemently distends the parts of that water, as he taught in the like case upon the 20 Experiment. But since we have already consider'd his Animadversion upon that, although this presumed distension of the water is not visible that we have observ'd, when cold water, that has been first freed from his interspers'd Air, is put into the Receiver, notwithstanding that the *Funiculus* should in that case also distend it; we are so afraid of tiring out the Readers patience by the frequent repetition of the same things, that we will leave it to him to judge which of the two Explications, the Examiner's or ours, is to be preferred, without troubling him and our selves with defence of Accounts against which our Adversary does not here make any peculiar Objections.

And thus have we by God's assistance considered what the Examiner hath been pleas'd to oppose either against our particular Explications, or against the *Hypotheses* that divers of them suppose: Wherein I have been the more particular and prolix, because I would willingly excuse my self and others from the trouble of any more Disputes of this kind. I hope there is not in my Answers any thing of Asperity to be met with; for I have no quarrel to the Person of the Author, or his just reputation; nor did I intend to use any more freedom of Speech in the answering his Objections, than his resolute way of proposing divers of them made it on those occasions needfull for the Caution of those Readers who are not acquainted with our differing ways of writing, and perhaps have not observ'd that some men are wont to consider as much what they propose but with a *Perhaps*, or some such expression of diffidence, as others do what they deliver far more resolutely. And though being very far from being wedded to my Opinions, I am still ready to exchange them for better, if they shall be duly made out to me, (which I think it possible enough they may hereafter be;) yet peradventure the Reader will think with me, that the Examiner has

has not given me cause to renounce any of them, since the Objections he has propos'd against me have been sufficiently answered, and since the *Hypothesis* he would substitute in the room of ours (besides that it is partly precarious) supposes things which divers of the eminentest Wits of our Age (otherwise of differing Opinions) profess they cannot admit or so much as understand: Whereas the Weight and Spring of the Air are not denied by our Author himself, and are demonstrable by Experiments that are not controverted betwixt us. Which things I represent for the defence of what I think the Truth, and not to offend my learned Adversary, who shall have my free consent to be thought to have fail'd rather in the Choice than in the Management of the Controversie. Though since this passes for his first Book, and since consequently he is not like to have been provoked, or engaged in point of Reputation, to challenge me or any of those far more eminent Persons he has nam'd among his Adversaries, I am induc'd by the severity wherewith I have known eminent *Virtuosi* speak of his Attempts, and particularly of his *Funiculus*, to fear that some of those he has needlessly oppos'd, will be apt to apply to him that of St. *Austin* against some of his Adversaries, that had disputed against him with much more Subtilty than Reason, *In mala causa non possunt aliter, at malam causam quis eos cogit habere?* But this notwithstanding I am, as I was going to say, content my Adversary should be thought to have said for his Principles as much as the Subject will bear; nor would I have it made his Disparagement, that I have declared that his whole Book has not made me depart from any of my Opinions or Explications, since his *Hypothesis* and mine being inconsistent, it may be looked upon as a sign rather that each of us have, than that either of us have not, reason'd closely to his own Principles, that the things we infer from our contrary Suppositions do so generally disagree.

R r

F I N I S.

AN EXPLICATION OF RAREFACTION.

THE chief Arguments of the Author of a certain Treatise *De Corporum inseparabilitate*, whereby he endeavours to invalidate the *Hypothesis* of the Weight and Spring of the Air, and to set up and establish instead thereof an unintelligible *Hypothesis* of Attraction, performed by I know not what strange imaginary *Funiculus*, are only Five, two against the former, and three for the later. The first of which is, That the Weight and Spring of the Air are not sufficient to perform the Effects ascribed to them: The second, that could they be performed by that *Hypothesis* granted, yet the way of this strange Spring it self is not intelligibly explained or explicable by the Defenders of it. Now the former of these being little else but a bare Affirmation, and the later bearing some shew of Demonstration, I shall endeavour to examine it as I find it set down in his 20, 21, 22, 23 and 24 Chapters, to which (especially the 23) he very often in his Book refers the Reader for satisfaction, pretending there to evince that Rarefaction cannot be made out any otherwise than by supposing a body to be in 2, 3, 4, 10, 100, 1000, 1000000 of places at the same instant, and adequately to fill all and every one of those places.

First therefore, we will examine his Negative, and next his Affirmative, Arguments for this strange *Hypothesis*.

His Negative I find in the 20 Chapter, where he endeavours to confute the two ways of explicating the Rarefaction and Spring of the Air, namely, that of the *Vacuists* and that of the *Plenists*.

Concerning the first of these we find him conclude it impossible

sible, first, because he had before proved that there can be no *Vacuum*, which being done by a Circle (*viz.* There is no *Vacuum* in the Tube because Nature abhors a *Vacuum*, and we see Nature abhors a *Vacuum* because she will not suffer a *Vacuum* in the Tube above the *Mercury*, but to prevent it will continually spin the Quicksilver into *superficies*, and never diminish the body of it) will suffer me to pass to his next, which is, That this way is false, because in the Experiment of the Carp's Bladder the Air is rarefied a 1000 times bigger; nay, in respect of the body of Gold it has 1000000 times less matter in equal spaces. And this, says he, is a *Phænomenon* that is impossible ever to be made out by interspers'd Vacuities. Now that the *Vacuists* cannot presently, by so bold an assertion as this, be made to forsake their Principles, he may perceive by these following Solutions which I shall give of all the *Phænomena* he recites, flowing naturally from an *Hypothesis* that I shall for the present assume. Let us suppose then the Particles of Bodies, at least those of the Air, to be of the form of a piece of Ribond, that is, to be very long, slender, thin and flexible *lamina*, coyled or wound up together as a Cable, piece of Ribond, Spring of a Watch, Hoop, or the like, are: We will suppose these to have all of them the same length, but some to have a stronger, others a weaker Spring: We will further suppose each of these so coyled up to have such an innate circular motion, as that thereby they may describe a Sphere equal in Diameter to their own, much after the manner that a Meridian turn'd about the Poles of a Globe will describe by its revolution a Sphere of the same Diameter with its own in the Air. By this Circular motion the parts of the *lamina* endeavouring to recede from the Centre or Axis of their motion, acquire a Springiness outward like that of a Watch-Spring, and would naturally flie abroad until they were stretch'd out at length, but that being incompast with the like on every side, they cannot do it without the removal of them, as not having room sufficient for such a motion. And the faster this circular motion is, the more do the parts endeavour to recede

from the Axis, and consequently the stronger is their Spring or endeavour outward. These springy Bodies thus shap'd and thus moved are sufficient to produce all the *Phænomena* he names as impossible to be explicated. And, first, for the business of Expansion, it will very naturally be explained by it: As let us suppose for instance the Diameter of these small coyled Particles of the Air (which being next the Earth are press'd upon by all those numerous incumbent Particles that make up the Atmosphere, and are thereby so croud'd that they can but very little untwist themselves; let us suppose, I say, the Diameter of these Particles) to be $\frac{1}{10000000000}$ of an Inch; and then to be much of the form of those represented in the 4 Figure by *ABCD*: and that these Particles, when a considerable quantity of the pressure of the ambient parts is taken away, will flie abroad into a Coyle or Zone ten times as big in Diameter as before; that is, they will now be $\frac{10}{10000000000}$ of an Inch in Diameter, and appear in the form of those in the Figure express'd by *EFGH*: these Zones whirld round as the former will describe a Sphere 1000 times as big in bulk, and thereby fence that space from being entred by any of the like Zones: this it would doe, supposing those Spheres did immediately always touch each other; but because of their circular motion, whenever they meet they must necessarily be beaten, and flie off from one another, and so require a yet greater space to perform their motion in. This suppos'd, there are no *Phænomena* of Rarefaction (which is enough at present to answer what he objects) but may be naturally and intelligibly made out. As first, for that of the swelling of a Carp's Bladder, if we suppose some small parcels of the former comprẽss'd *lamina* to lie latitant within the folds of it, and being much coyled up together scarce to take any sensible room; this Bladder in the Air will appear to contain very little or nothing within it; whereas when the pressure of the Air is taken off in good part from the outides of it, then those formerly latitant Particles disclose themselves by flying open into much bigger Zones, so as perhaps to be able to defend a thousand

thousand times bigger space from being entred into by their like or any other gross Particles, such as those of the Bladder. Now because the Pores of a Bladder are such as are not easily permeable by the Particles of Air, therefore these lurking Particles so expanding themselves must necessarily plump out the sides of the Bladder, and so keep them turgid until the pressure of the Air that at first coyled them be re-admitted to doe the same thing for them again.

Next, as for Rarefaction by heat, that will as naturally follow as the former from this *Hypothesis*. For the Atoms of fire flowing in in great numbers, and passing through with a very rapid motion, must needs accelerate the motion of these Particles, from which acceleration their Spring or endeavour outward will be augmented, that is, those Zones will have a strong nitency to flie wider open, (for we know that the swifter any body is moved circularly, the more do the parts of it endeavour to recede from the Centre of that motion) from whence if it has room will follow a Rarefaction. As for the conveyance of Light, that being according to *Epicurus* performed by the local motion of peculiar Atoms, their motions to and fro through this *medium* will be less impeded by the rarefied Air than by the condens'd; as indeed upon Experiment we shall really find them.

As for his third Objection drawn from his supposed attractive virtue of the thus rarefied Air, that is quickly answered, by denying it to have any power at all of attraction; and by shewing (which is already done) that what effects he would have to be performed by the attraction of the included, is really done by the pressure of the ambient, Air.

And, lastly, the *Phenomena* of my Lord Bacon's Experiment are sufficiently obvious and easie to be deduc'd.

So then, by granting *Epicurus* his Principles, that the Atoms or Particles of bodies have an innate motion; and granting our Supposition of the determinate motion and figure of the Aërial Particles, all the *Phenomena* of Rarefaction and Condensation, of Light, Sound, Heat, &c. will naturally and necessarily fol-

low: and the Author's Objections against this first way of Rarefaction will signifie very little.

As to the second way of Rarefaction by the intrusion or intervention of some subtile matter or *Aether* into the spaces deserted by the rarefying Particles, which is that propos'd by the Assertors of a *Plenum*, this also is by the Author condemned, and branded with Impossibility. And why? First, because 'tis (he says) impossible that the above-mentioned *Phænomena* of the Carp's Bladder can be explained by it. Secondly, because 'tis impossible to give a reason from it of the impetuous ascent of Water admitted into an exhausted Receiver. And, Thirdly, because 'tis impossible to explicate the *Phænomena* of Gun-powder. His Reasons to confirm which three Impossibilities, because drawn from a more mistake, or ignorance of those *Hypotheses* which have been invented by the Assertors of that Opinion, I shall pass over, and content my self to explain a way how these Impossibilities may become Possibilities, if not Probabilities.

And the way that I shall take, shall be that of the most acute Modern Philosopher Monsieur *Des Cartes*, published in his Philosophical Works: Which is this, That the Air is a Body consisting of long, slender, flexible Particles, agitated or whirl'd round by the rapid motion of the *Globuli Caelestes*, and the subtile Matter of his first Element, whereby they are each of them enabled to drive or force out of their Vortice all such other agitated Particles. Now the swifter these Bodies are whirl'd round, the more do their flexible parts flie asunder and stretch themselves out, and the more forcibly do they resist the ingress of any other so agitated particles into their Vortice, and consequently the slower their motion is, the less will be their resistance. And because there is a vast number of these whirled Particles lying one above another, and each Particle having its peculiar gravity; it will necessarily follow that the undermost (which to maintain their Vortice must resist so great a pressure) must very much be hindered from expanding themselves so far as otherwise they would, were there none of those

incom-

incompassing agitated Particles that lay in their way: And that those being by any means removed, or they themselves by a more rapid motion of the Particles of their Vehicles, the first and second Element, (which is according to that *Hypothesis* an effect of Heat) more swiftly and strongly whirled round, they presently begin to expand themselves, and maintain a bigger Vortice than before. Now to perform what I just now promised, I shall endeavour to give a possible, if not a probable, cause of the objected *Phænomena*. And, First, for that of the Carp's Bladder, where the Air is rarefied (says the Author) 1000 times, it will easily be explained by supposing the few Particles of the Air, which (whilst they sustain the pressure of all the incumbent Atmosphere) inconspicuously lurk within the Bladder, (each of them being able to maintain but a very small Vortice) to be by the subsiding *Mercury* in the *Torricellian* Experiment freed from the pressure of the Air, and their motion continuing the same (by reason that the Transcursion of their Vehicles is not at all or very little hindered either by the Glass or Bladder) their parts having room to expand themselves, will flie abroad to such Extensions as may perhaps make a Vortice 1000 times as big in bulk as what they were not able just before to exceed. Hence the Particles of the Air (being so gross as not easily to pervade the Pores of the Bladder) must necessarily drive out the sides of the Bladder to its utmost extent, and serve to fill the Receiver in the *Magdeburgick* Experiment. Now, whereas these Particles will by the same pressure of the Air be reduc'd to the same state they were in at first, that is, to be thronged into a very little room, and thereby be able to maintain a very small Vortice; the Air let in in the *Torricellian* Experiment reduces the Air in the Bladder to its former inconspicuousness, as the admission of the Water in the *Magdeburg* Experiment does that Receiver full of rarefied Air into the bigness of a Hazel Nut. Now the Water in this last-mention'd Experiment enters with a great impetuosity, because driven on with the whole pressure of the Atmosphere, and resisted only by the small force of the so-far-rarefied Air.

As:

As for the Author's Objection against this way of Rarefaction drawn from the *Phænomena* of Gun-powder, I shall endeavour to answer it by shewing them possibly explicable by a *Cartesian Hypothesis*. For supposing those Terrestrial parts of the Gun-powder to be first at rest, and afterwards agitated by the rapid motion of his first Element, there will be sufficient difference of the former and later condition in respect of Extension; and supposing the particular constitution of Gun-powder (arising partly from the Specifick forms of the Particles of its ingredients, Nitre, Sulphure and Char-coal, and partly from their proportionate commistion) to be such as will readily yield to the motion of his *Materia subtilis*, so soon as an ingress is admitted to it by the firing of any particular parcel of it, the Expansion will be speedy enough.

So then let us suppose a Barrel of Gun-powder placed in some close room, to some grains of which we will suppose some actual fire to be applied, by which actual fire (the Texture of the Powder being such) those grains are suddenly fired, that is, many Millions of parts, which before lay still and at rest, are by the action of the burning Coals shatter'd, as it were, and put into a posture ready to be agitated by the rapid motion of the *Materia subtilis*: into which posture they are no sooner put, than agitated and whirled sufficiently by it; whence follows a vast Expansion of that part of Gun-powder so fired. For each of its parts being thus whirld and hurried round, expel and beat off with great violence all the contiguous Particles, so as that each Particle takes up now 1000 times as much Elbow-room (if I may so speak) as just before serv'd its turn, and consequently those that are outermost take every one their way directly from the parcel or Corn they had lain quiet in, being hurried away by the sudden Expansion of the Particles that lay next within them; so that whatever grain or parcel of Gun-powder they chance to meet with, before they have lost their motion, they presently shiver and put into such a motion as makes them fit to receive the action of the *Materia subtilis*. Which subtile Matter being every where present, and

and nothing slow in performing its office, immediately agitates those also like the former; so that in a trice the Particles of the whole Barrel of Gun-powder are thus disordered, and by the motion the *Materia subtilis* must needs be hurried away with so great an impetuosity on all sides, as not only to break in pieces its slight wooden prison, and remove the lighter Particles of the ambient Air, but huge Beams, nay, vast accumulated Masses of the most compacted Structures of Stone, and even shake the very Earth it self, or whatever else stands in its way, whose Texture is so close as not to give its Particles free passage through its Pores. This understood, I see not, first, what the Author's three Arguments brought to prove his Objection signifie, for there are no more Corpuscles in the room before the Gun-powder is fired than after, nor is there any more matter or substance before the sides of the room by yielding give place for the external fluid Bodies to succeed, and the only change is this, that the *Globuli secundi Elementi* (as he calls them) are expell'd out of the room, and the *Materia primi Elementi* succeeds in the place of it. Nor do I see, secondly, what great reason he had for his grand Conclusion, *Hæc abundè demonstrant, rarefactionem per hujusmodi corpuscula nullatenus posse explicari.*

Having thus examined the Author's first Arguments, that Rarefaction cannot be made out by any other way than his; we shall find his other, which he brings to establish his own *Hypothesis*, much of the same kind. As, First, that his way of Rarefaction implies no Contradiction: For if the affirming a body to be really and totally in this place, and at the same time to be really and wholly in another, that is, to be in this place, and not to be in this place, be not a Contradiction, I know not what is. Next, that some learned School-men have thought so; to which I answer, more learned men have thought otherwise. And, lastly, that there are very plain Examples of the like nature to be found in other things; of which he only brings one, *viz.* that of the *Rota Aristotelica*, which upon

examination we shall find to make as little to the purpose as any of the other.

An Explication of the Rota Aristotelica.

THe great Problem of the *Rota Aristotelica*, by his explication of which he pretends not only to solve all the difficulties concerning Local motion, *quæ Philosophorum ingenia hactenus valde exercuerunt*, but to give an instance for the confirmation of his unintelligible *Hypothesis* of Rarefaction, wherein there is *extensio seu correspondentia ejusdem rei ad locum nunc majorem, nunc minorem*; we may upon examination find to be either a Paralogism, or else nothing but what those Philosophers said whom he accounts gravel'd with it. Of this Subject he begins in his 25th Chapter, where after he has set down a description of it, he makes an instance in a Cart-wheel; *Rem ante oculos ponit rota alienius currus, ejusq; umbo seu lignum illud crassum & rotundum cui insiguntur radii; siquidem dum progrediente currum ipsa rota circumducta describit in subjecta terra orbitam sibi æqualem, umbo ille describit in subjecto aëre orbitam* (I suppose both here and before he means *Lineam*) *se multo longiorem, utpote æqualem orbitæ totius rotæ, licet ipse non nisi semel quoq; fuerit circumvolutus.* (As for what he says, that the Nave must be suppos'd to pass through the Air, and not to touch a solid Plain, I do not yet understand the force of his Reason, nor why he sets it down, making nothing to his present purpose, unless it were because he did not well understand the thing) In which, says he, the great difficulty is to explain how the Nave should be so turned about its Axis, *ut partes suas successive applicet lineæ duplo plures partes habenti, idq; motu perpetuo ac uniformi vel ad oculum instar interrupto.* Which how true, and what great occasion he had to wonder at the solution of that Problem by the Example of a man standing still and another walking, we shall find by and by, when we come to explain the Problem: But first I shall examine his *Hypothesis* and Explication.

tion. And First, he supposes Time to consist of a determinate number of Indivisibles; (that is, such as have neither *prius* nor *posterius* included in them) which he calls Instants. And next he supposes the *presentiam localem seu ubicationem cujuslibet partis indivisibilis & virtualiter extensa esse quoq; indivisibilem & virtualiter extensam*: Which supposition so strangely express'd is no more than this, that the extension or space of his Indivisibles is also indivisible. But as for his Virtual Extension, I confess I understand as little what it is as I verily believe he did; and therefore I will proceed to his following supposition. His Third therefore is, That by how much more rare a body is, by so much the more are its Indivisibles virtually extended. Hence his Fourth is, That though these Indivisibles be really indivisible, yet they are virtually *in quotvis partes divisibiles*. Whence he deduces his Fifth Principle, That since these Indivisibles are really indivisible and virtually extended, they must necessarily be moved after the same manner that other indivisible and virtually-extended things are. His Instances are in the motions of an Angel and an indivisible piece of wood, which, he says, are both of the same kind. As for that of Angels, having no immediate Revelation, and a Spirit and its actions not falling under sense, and not having any third way by which to be inform'd, I shall leave him there to enjoy his fancies. But as for that of his piece of wood, we shall find it sufficiently full of absurdities and contradictions. And first, he calls it indivisible, but why I know not; for 'tis neither really nor yet mentally so: not mentally so by his fourth Principle, where he says that his *virtualiter in quotvis partes divisibiles*, by which word *virtualiter* he means the same thing with *mentaliter*, or nothing. Nor, secondly, is it really so: for then (according to the main business of his Book, as may be gathered from the first words of his Title Page. *Tractatus de Corporum Inseparabilitate*) it would be impossible that any thing in the world should be divisible; for he making an inseparable continuity, and that Bodies will rather be (I can't tell how) stretch'd beyond their own dimension *in infinitum*, than part from one another;

a body may as soon pass through the dimensions of any one Indivisible, as pass between two. Next, he grants in the strange stretching or rarefaction of these Indivisibles a temporary motion of the condens'd Dimension; whence there will follow that there must be distinct places or *Ubi's*, it must be *terminus à quo, terminus ad quem, & medium*. And next, it were impossible to divide a line into two parts, supposing it consisted of an unequal number of Indivisibles; as if 101 Indivisibles of exceedingly-rarefied Air should be extended in length an Inch, it were impossible to divide that Inch into two equal parts. I might run over many more, but it would be too tedious to be here recited. As for his indivisible parts of Time, those also must necessarily be *in quotvis partes divisibiles*; for else the same body or Indivisible must necessarily be in divers places at the same instant. But because he can swallow, nay confidently affirm, this and many other such like contradictions and absurdities, I am not willing to mention them; and I think it would have made more for the Author's reputation if he had done so too. As for his last Chapter, where he applies these Principles to the Explication of the *Rota Aristotelica*, I have not here time to set down all the absurdities that any one that has but a smattering in the Mathematicks may observe: as, sometimes half an indivisible part of a Circumference may touch an indivisible of a Line; sometimes one may touch half, a quarter, a hundredth part, a whole one, two, ten, a hundred, &c. at the same instant; nay, an indivisible of a Circle may be all of it in a thousand places together, and the like. And this he brings as a great Argument to establish his *Hypothesis* of Rarefaction, pretending it to comprise many *Ænigma's* and very great difficulties; whereas the thing is very plain and easie, and contains no such obscurities. For if, for example, we suppose a Wheel *ABCD* to be moved in a direct motion from *AIC* to *KLM*, every point of it retaining the same position to that line that they had at the beginning of their motion, each of the points *AEIGC* will on a Plain, or in the *Medium* it pervades, pass through or describe a line equal to the line *IL*, and not only
all

all the points lying in the line AIC , but all and every point of the whole *Area* of the Circle; this must necessarily happen if the Diameter AIC be moved parallel to it self: But if whilst it be thus moved with an equal progression, it be likewise moved with an equal circulation, the case will be altered. For then, first, each point will by this compound motion describe on the Plain or *Medium* either a perfect Cyclorid, as when the Wheel makes one perfect revolution, whilst the whole is progressively moved from I to L ; or some Piece, as when the Wheel has not perfected its revolution; or more than a whole one, as when the Circle has made more than one whole revolution whilst it is moved in its determinate length. I shall here only consider the first, as pertaining more especially to my present purpose, and in regard the two later on occasion may be easily explicated by it. Next, each point of this Circle acquires from its compounded motion various degrees of Celerity as to its progression, according to its various position to a point which is always found in some part of the line drawn through the Centre of the circular motion perpendicular to the progressive. And it is found thus, as the Circumference to the *Radius*, so is the line of the progressive motion to the distance of the point from the Centre. And this happens because the line of Progression is equal to the Circle described on that distance as *Radius*; each point therefore of this smaller Circle, when it comes to touch the Perpendicular, must, as to its progressive motion, stand still: This point therefore will be the Centre of this compounded motion. Now because for the explication of the *Rota Aristotelica* we need not consider any other than those Points which are transient through or cross the Perpendicular line, we shall only examine them. Let then in our Example A be the Centre or immoveable point, the Circumference therefore $ABCD$ will be equal to IL or AK by our *Hypothesis*. Now because the point I , which is the Centre of the Rotation, has only one motion, *viz.* that of Lation, its celerity will be equal to the single celerity of the Lation; we will therefore put it to have one degree, C , because it is moved with two motions, both tending the same

way, and each equal to the velocity of *I*, must needs have two degrees of velocity. The point *F*, because moved with two motions, both tending the same way, the one (*viz.* its Lation) being equal to that of *I*, and the other (because it is but half as far distant from the Centre of Rotation as *C*, and therefore is moved but with half the celerity of *C*, which was equal to that of *I*) but half as quick, we will put to have one degree and an half. By the like method we might find the velocity of all the points in the Perpendicular, *viz.* such as we have there marked some of them; but it would be too tedious, we needing not to consider more than the two points *A* and *E*. The point at *E* being moved forward by its progression with the same velocity that *I*, but by its rotation (which is but half as swift as that of the Circle *ABCD*, that is double the Circle *EFGH*) being moved the contrary way or backwards with half the velocity, loseth half of its progression forwards. The point in *A* being by its progression moved forwards equally swift with *I*, and by its rotation (the Circle *ABCD* being equal to the line *IL*) being carried backwards with equal velocity, must necessarily stand still as to its progression. Now having shewn that the point *A* (being by reason of its two equal opposite motions at rest) does only touch a point of the line *AK*, and is not at all moved on it; and that the point *E* (being carried forward twice as fast by its progression as it is carried backward by its rotation, and thereby moved half as fast as the point *I*) does not only touch the line *EK*, but whilst it touches it is moved on it with a progressive motion half as swift as that of *I*: it will necessarily follow, that each point situate in *E* must necessarily describe a small line, which is a part of the whole *EC*. Now both the contact of the former, and the contact and progression of the latter, being performed by an infinite succession of points in the space of an infinite succession of Instants; I see not any one difficulty of this Problem but may satisfactorily be given an account of by it, without having recourse to the *Hypothesis* of the determinate number of indivisibles of space and time, which at least will only come to this, that *Is such a de-*
terminate

terminate moment or minute space of time, (which consists of an infinite consecution of Instants, and has *prius* and *posterius* in it; though yet he will call it an Instant, and have it to have the same proprieties with an Instant used in the common Philosophical sense) *such a determinate minute Corpuscle* (which, though it have extension in length, breadth and thickness, yet will he not admit it to be divisible or have parts, no not though, according to his *Hypothesis*, the indivisible of one body may be rarefied to be as big in bulk as a million of the indivisibles of another, but will have it to be called and to be a real indivisible) *will successively pass over such a determinate space or length* (which yet he will not admit to be divisible, though according to his Principles it may equalize the length of millions of his other Indivisibles, nor admit a successive motion, but instantaneous, though that does necessarily put a body into two, three, ten, a hundred, &c. places at once; but will have these also to be indivisible.) Haste makes me pass over the absurdities about the contact of a Circle and a Line, and to comprise in short all that great Explication he has given of this and other intricate (as he calls them) Problems, which is this, That the reason of the celerity of the motion of some one of these indivisibles above another is, that it passes through a greater part of an Indivisible in the same instant than the slower; that is in plain sense no more than this, One body is swifter than another because it is moved faster. From whence he draws several Corollaries, as that Hence may be given a reason why an Eagle is swifter than a Tortoise, *viz.* because it moves faster. I should have solved several Objections which may be brought against the divisibility of Quantity *in infinitum*; but that as all the Scholastick Writers are full of them, so it is a Subject which we are least able to dispute of having very little information of the nature of Infinity from the Senses.

THE
JOURNAL
OF
THE
AMERICAN
MEDICAL
ASSOCIATION
PUBLISHED WEEKLY
CHICAGO, ILL.
1912

Vol. 45, No. 12, December 12, 1912

CONTENTS

ORIGINAL ARTICLES

THE
JOURNAL
OF
THE
AMERICAN
MEDICAL
ASSOCIATION
PUBLISHED WEEKLY
CHICAGO, ILL.
1912

The Citations Englished.

CHap. 2. Pag. 3. *Cum tota vis, &c.* Being the whole power of the Spring of the Air depends upon the *Æquilibrium* of its weight with twenty nine Inches and an half of Quicksilver, so that this Spring doth neither more nor less in a shut place, than is done by that *Æquilibrium* in an open place: It is manifest, seeing we have shewed the *Æquilibrium* to be plainly fictitious and imaginary, that the Spring ascribed to the Air is so likewise.

P. 4. Nam si Tubus, &c. For if a Tube but twenty Inches long (such as we used in our first Argument) be not quite filled with Quicksilver, as before, but a little space be left betwixt the *Mercury* and the Finger on the top of the Tube, in which Air only may abide: We shall find that the Finger below being removed, the Finger on the top will not only be drawn downwards, as before, but the Quicksilver shall descend also, and that notably, *viz.* as much as so small a parcel of Air can be extended by such a descending weight. So that if instead of Air, Water or any other Liquor which is not so easily extended be put in its place, there will be no descent at all.

Hence, I say, against this Opinion an Argument is framed: For if the external Air cannot keep up those twenty Inches of Quicksilver from descending, as we have proved; how shall it keep up twenty nine Inches and an half? Assuredly these can no way be reconciled.

Ibid. Dices fortè, &c. You will perchance say, that the Quicksilver therefore doth in the alledged case descend, because it is thrust down by that parcel of Air which dilates its self by its own Spring.

Ibid. Sic deberet, &c. So should the Finger be rather thrust from the top of the Tube, than thereby fastned to it; because this Dilatation must be made as well upwards as downwards.

P. 6. Concupi, &c. It cannot be conceived how that Air should dilate it self, or thrust down the *Mercury*, unless by taking up a greater place; which thing these Authors are much against, asserting that Rarefaction can be made no otherways than by Corpuscles or Vacuities.

Chap. 3. p. 7. Si, &c. If you take a Tube open at both ends of a good length; suppose forty Inches long, and fill it with *Mercury*, and place your Finger on the top as before, taking away your lower Finger you will find the *Mercury* to descend even to its wonted station, and your Finger on the top to be strongly drawn within the Tube, and to stick close unto it. Whence again it is evidently concluded that the *Mercury* placed

in its own station is not there upheld by the external Air, but suspended by a certain internal *Cold*, whose upper end being fastned to the Finger draws and fastens it after this manner into the Tube.

Chap. 4. p. 8. *Sumatur, &c.* Take a Tube shorter than twenty nine Inches and an half, for instance of twenty Digits, not shut, as hitherto, at one end, but with both ends open: let this Tube, its Orifice being immers'd in stagnant *Mercury*, and one Finger being plac'd underneath, that the *Mercury* to be poured in run not through, be filled with *Mercury*; and then another Finger be apply'd to its Orifice, to close it well: Which being done, if you draw away your lower Finger, the upper will be found to be strongly drawn and suck'd into the Tube, and so stily to adhere to it, (or rather to the Quicksilver, as I shall hereafter shew) that it will elevate the Tube it self with all the Quicksilver, and make it continue to hang pendulous in the Vessel.

From which Experiment this Opinion is most clearly refuted: For, seeing according to it the Quicksilver in such a Tube but twenty Inches long must be thrust upwards by the preponderating Air; it will never by it be explained how this Finger is so drawn downwards, and made so strongly to stick to the Tube. For it cannot by the Air thrusting upwards be thus drawn downwards.

p. 10. 11. *Quod vel, &c.* Which is thence confirmed, Because if that preponderating Air succeeds, as is asserted, in the place of the lower Finger which was withdrawn, that is, if it uphold the Quicksilver after the same manner which it was upheld by the lower Finger applied under it; it is manifest, according to this Opinion, that the Finger on the top ought not to be more drawn downwards after the lower Finger is removed than before. Seeing then that Experience teacheth the contrary, it is manifest that Opinion must be false.

Chap. 5. p. 11, 12. *Quarto, &c.* In the fourth place it is impugn'd, Because thence it would follow that Quicksilver through a like Tube might be suck'd with the same easiness out of a Vessel that Water is suck'd out of the same. Which notwithstanding is contrary to Experience, by which we are taught that Water is easily drawn into the mouth of him that sucks, whereas Quicksilver cannot be drawn thither by his utmost endeavour, nay, scarce unto the middle of the Tube.

The sequel I thus manifest: Because seeing, according to this Opinion, that the Liquor underneath, whether it be Water or *Mercury*, may so ascend, no more is requir'd but that the Air shut in the Tube may be drawn upwards by sucking; which being drawn up, the Liquor underneath will immediately ascend, being thrust thither by the external Air now preponderating, (as *Paequet* declares in his Anatomical Discourse, p. 63.) It is manifest

manifest that the *Mercury* may be suck'd out with the same easiness that Water is suck'd out with. Which being so evidently against Experience, the Opinion from whence it is deduced must needs be false.

p. 13. *Neg; hoc, &c.* And not only this, but over and above, if a Glass *Diabetes* or Syringe be made of a sufficient length, and after that the Sucker is thrust into the utmost Orifice, it be placed according to use in the *Mercury* underneath; he finds that as soon as the Sucker is drawn out, the *Mercury* follows, and ascends to the same height of two Feet and three Inches and an half. And when afterwards, although no greater force be added, the Sucker is drawn higher, he finds that the *Mercury* stands, and follows no further, and so that space is made empty which remains between the *Mercury* and the Sucker.

p. 15. *Maneat igitur, &c.* Be it therefore confirm'd by so many Arguments, of which every one is sufficient in it self; that Quicksilver (the Experiment being made in an open place) is not upheld from falling by the weight of the external Air.

Cap. 6. *Ibid. Argentum, &c.* That Quicksilver in a close place is not upheld from falling by the *Elastor* or Spring of the Air.

Ibid. Cum ita, &c. Seeing the whole power of this Spring depends upon the already-confuted *Equilibrium* of the Air with 29 Inches and an half of Quicksilver, so that this Spring does neither more nor less in a close place than is done by that *Equilibrium* in an open place; it is manifest, seeing this *Equilibrium* is already shewn to be plainly fictitious and imaginary, that the Spring of the Air is so likewise.

p. 16. *Nec placet, &c.* And that this Spring doth neither more nor less in a close place, than is done by that *Equilibrium* in an open place.

Ibid. Adde, &c. Add, that seeing the Experiments brought in the Chapter above of the adhesion of the Finger, &c. are alike in a close and an open place: it is necessary and certain that the same Arguments made against the *Equilibrium* have force against the Spring of the Air.

p. 17. *Et profecto, &c.* And really if these Authors would consider how great a difficulty there is in explaining this Spring of the Air, unless the same Air by it self alone may take up a greater place, I believe they would readily alter their Opinion.

Part 2. Chap. 1. p. 19. *Constat hoc, &c.* This appears from what has been already spoken in the preceding Chapter: For the Quicksilver descending cannot so draw the Finger downwards, and suck it unto the Tube, unless it be hung upon the Finger by such a Cord, which by its weight it vehemently stretches; as is manifest by it self.

Ibid. Respondit, &c. I answer, That this comes to pass that there may be no Vacuity, seeing there is nothing else there that can succeed into the place of the descending Quicksilver.

Ibid. And hence is confirm'd that common Axiom used in the Schools for so many Ages past, that *Nature doth abhor a Vacuum.*

Ibid. *Nam licet, &c.* For though the immediate cause why Water (for instance) doth not descend from a Gardener's Watering-pot (for that Example they use) stopt on the top, is not the fear of a *Vacuum*, but the reason now mentioned, namely, That there is not weight sufficient to loose that conjunction by which the Water doth adhere to the top of the closed Water-pot: Nevertheless in the end we must of necessity come to that Cause.

p.20. *Qua quidem, &c.* Which traction and adhesion when it cannot proceed but from some real Body placed between the Finger and the *Mercury*, it is manifest that that space is not empty, but filled with some true substance.

Ibid. *Et quod, &c.* Because no visual species's could proceed either from it, or through it, unto the eye.

Ibid. *Vera, &c.* To be filled with any true substance.

p.24. *Huc etiam, &c.* And to this purpose make those considerable Vibrations with which Quicksilver is stirred in its descent: For the same thing happens here that befalls other *Pendula* in their fall from on high.

p.27. *Argentum dum, &c.* Quicksilver while it fills the whole Tube doth not only touch its top, (as you would think at the first sight) but doth firmly stick unto it also; as it is manifest from the Experiment mentioned in the first Argument of the third Chapter, concerning the Tube open at both ends.

Ibid. *Litter illud, &c.* Though that Orifice of the Tube be anointed with Oyl, or any other matter that will hinder adhesion, nevertheless the Finger will no less firmly stick than before.

p.28. *Partes, &c.* That the parts of Air it self so shut up in the Tube (which otherwise are so easily severed) are now so firmly glued to one another, that they make (as we see) a strong Chain by which not only Water but even weighty Quicksilver is drawn on high.

Ibid. *Rarefactionem, &c.* That the Rarefaction or Extension of a Body so as to make it take up more space is not only made by Heat, but by distension or a certain *disjoining power*: as on the contrary Condensation is not only made by Cold, but also by Compression, as infinite Examples bear us witness.

p.29. *Cum per, &c.* Seeing by the first Note is manifest that the Quick-silver doth so stick to the top of the Tube, and by the second Note the Rarefaction is made only by the mere distension of the body, it so comes to pass that the descending Quicksilver leaves its external or upper *superficies* fixed unto the top of the Tube, and by its weight doth so stretch

and extenuate it, untill it becomes easier to leave another *superficies* in like manner, than to extend that any further. It leaves therefore a second, and doth by its descent extend that a little further, until it becomes easier to separate a third than to extend that any further: And so forwards, until at length it hath no power to separate or extend any more *superficies*, namely, until it comes unto the height of 29 Inches and an half; where it acquiesces, as we have declared in the first Chapter.

p.30. These Surfaces seem to be separated from the Quicksilver, and to be extended into a most slender string by the weight that falls down, after the same manner that in a lighted Candle surfaces of like sort are separated from the Wax or Tallow underneath by the heat above, and are extenuated into a most subtle flame. In which it is worth observation, that as that flame doth doubtless take up more than a thousand times a greater space than the part of the Wax of which the flame was made took up: So is it here to be thought, that that string doth take up a space more than a thousand times as big as that which the small particle of Mercury, from whence it arose, did before take up. As also it doubtless happens when such a particle by a fire underneath is turned into a vapour.

p.36. *Corpore, &c.* A body taking up a place, for instance, twice as big as it self; it is of necessity that every part of it must likewise take up a place twice as big as it self.

p.41. *Juxta, &c.* According to the more probable Opinion such a virtual extension of a corporeal Being is not to be granted, as being only proper to such as are Spiritual.

Ibid. Prestas, &c. It is better to continue in the common Opinion; which hath been hitherto received In the Schools; which although it doth not clearly resolve all difficulties, yet it doth not openly lie under them.

Ibid. Necessario, &c. We must needs confess that one and the same part must be in two places adequately. For seeing it is indivisible, and takes up a greater place than before, it must of necessity be all in every point of that place, or that be virtually extended through all that space.

p.43. *Cumtempus, &c.* Seeing Time is a Being essentially successive, so that neither by divine power can two of its parts exist together.

p.44. *Respondet, &c.* I answer, that all these things happen because the Gun-powder so kindled and turned into flame takes up a much greater space than before. Whence it comes to pass that seeing the Chamber was before quite full, by this means the walls are broken that there may be no penetration of bodies.

p.48. *Partim, &c.* Sometimes within the Chapel, sometimes in the open Air, the wind sometimes blowing, and sometimes being still.

p. 52. *Sed dict, &c.* But it may be said, that on the top of the Mountain it therefore descended after that manner, because the Air was more cold there, or of some other temperature, such as might cause this descent.

p. 68. *Hoc esse, &c.* That this is the difference between Pressure and Suction, that Suction makes such an adhesion, and Pressure doth not.

p. 69. *Hoc quousq, &c.* And even this Experiment doth very well agree with our Principles: For seeing by this depression of the Sucker, the Air shut up in the cavity of the Cylinder is separated from the Cylinder, and doth descend together with the Sucker, (as we have, *Chap. 13.* observed of Water descending together with Quicksilver) it comes to pass that in that whole depression new surfaces are taken from that descending Air, and stretched out; as we have there explained it in the case of descending water: Since therefore such surfaces are as easily slip'd of and extended in the end of the depression as in the beginning; it is no wonder that there is found the same difficulty of depressing it at both times.

Ibid. Eo magis, &c. That the Air is so much the more extended and rarefied, by how much the more is thence exhausted, and so doth acquire a greater force of contracting it self.

p. 71. *Ac pressio, &c.* But truly it seems not credible that this most soft Air should so vehemently compress a Glass on all sides (especially one of that thickness there mention'd) as to break it.

p. 72. *Porro, &c.* It is therefore more truly answered, that the Glass is therefore so broken, because by that exclusion its sides are more vehemently drawn inwards than (by reason of the figure unfit for resistency) they were able to resist. For seeing the included Air doth most firmly stick to the sides of the Glass, to draw out the Air will be nothing else but to endeavour to bend the sides of the Glass inwards.

Ibid. Sed pressio, &c. But truly this seems too far remov'd from Truth, and may be by this alone sufficiently refuted. Because if the pressure of the Air which descends by that Tube into the Vial be so great as to break the Vial it self, it ought certainly, before the breaking of the Vial, very much to move the water in which the Tube is immerg'd, and to excite bubbles in it, &c. as appears, if any one blowing through that Tube doth make but an ordinary pressure upon the water. But it is sure that the water before the Vial is broken doth not move at all: as the Experimenters will find.

p. 73. *Idem, &c.* Though the Tube had been shut at the top the Vial had doubtless been broken after the same manner.

p. 74. *Sed respondet, &c.* But it is more rightly thence infer'd that the Cylinder doth nothing there before.

p. 75. *Idem, &c.* I say then that the Quicksilver doth by that exhaust-

on so descend in the Tube, because it is drawn downwards by the Air incumbent upon the restagnant Quicksilver: For that Incumbent Air, being by its exhaustion greatly rarefied and extended, vehemently contracts its self, and by this contraction doth endeavour to lift the restagnant Mercury out of its Vessel; whence it comes to pass that (the restagnant Mercury now less gravitating upon the bottom of its Vessel) the Quicksilver in the Tube must descend, as is manifest in it self: So that it is no wonder that, the external Air afterwards entring, the Quicksilver again ascends, seeing by that ingress the force which elevates the restagnant Quicksilver is weakened.

Ibid. *Aiq; bine, &c.* And hence is a reason also given of another thing which is there noted, namely, that by the violent intrusion of the external Air into the Receiver the Quicksilver ascended considerably above 29 Inches and an half. For as by the extraction of the Air the Quicksilver is depressed below its station, so by the intrusion of new Air it is elevated above it.

p.77. *Nam si, &c.* For if it were kept up by that, it ought rather to ascend than descend in colder weather, because the Air then would be more dense and heavy. Therefore the Quicksilver is not upheld by the Equilibrium of Air, as is asserted.

p.78. *Hinc fit, &c.* Hence it comes to pass, that this Funicle being contracted by the cold, the water doth ascend in cold weather; but doth descend in hot, because by heat the Funicle is dilated.

Ibid. *Ego certò, &c.* I truly do not doubt but there are some such occult causes, by which the slender Funicle that suspends (as we mentioned in the 10. Chapter) the Quicksilver in the Tube is sometimes lengthened, sometimes shortened, and so doth sometimes let down, and sometimes lift up the Quicksilver.

p.79. *In decimo nono, &c.* In the 19. he shews that water doth in the same manner descend upon the exhausting the Receiver, as he had shewn Quicksilver in the foregoing Chapter to descend. Of both which seeing there is the same cause, there is no reason we should any longer insist on this.

p.80. *Nam si, &c.* For if it were done so, these bubbles ought not so to have ascended from the bottom of the Vessel, (as it is asserted they did, both in this and the following Experiments that treat of bubbles) but from the upper part of the water, where they are less compressed; as it is apparently manifest.

p.81. *Respondeo, &c.* I answer that the water, upon that exhaustion of the Air, doth not so ascend of its own accord, but is violently drawn or lifted upwards by that rarefied Air contracting it self. For as water doth suffer

suffer some compression (as appears by experience) so here also it suffers some distension. And hence it is clearly manifest why these bubbles should arise rather from the bottom of the Vessel, than from the upper part of the water. For when that vehement suction doth endeavour to elevate the water from the bottom of the Vial, there arises there a certain subtile matter, which being turned into bubbles doth so ascend as is mentioned in the 15. Chapter and the 4. Experiment.

p.82. *Certum esse, &c.* It is certain that that Opinion is sufficiently refuted by this single Experiment.

p.83. *Necesse, &c.* It must needs be that that stone could not otherwise descend, than by leaving behind it such a thin substance as is left by Quicksilver or Water descending in like manner.

Ibid. *Unde, &c.* Whence I plainly conceive that if two perfectly-polish'd Marbles were so joyned that no Air at all were left between them, they could not be drawn asunder by all the power of Man.

Ibid. *Uti etiam, &c.* Which also is confirmed by the Example the Author there brings of a Brass Plate sticking so close to a Marble Table, that by a lusty Youth, who boasted of his own strength, it could not be lifted off by a Ring fixed to its Centre.

p.84. *Eodem, &c.* Almost the same manner as we see in Cupping-glasses applied to a Patients back, in which the flame being extinct, the rarefied Air contracting it self doth so vehemently (as we see) lift up, and draw the flesh within the Glass.

p.85. *In his, &c.* In these three there is nothing occurs to be peculiarly here explicated, the account of which is not easie from what is already delivered.

Ibid. *Existimo, &c.* But I think that Whiteness should be rather called a reflex than an innate light, because, as the Author bears witness, it appears not in the dark, but only in the day, or by Candle-light.

p.86. *Verum, &c.* But it seems impossible that such *Animals* should dye so soon only for want of a thicker Air.

p.87. *Quia per, &c.* Because by the self-contraction of the rarefied Air their breath is drawn out of their bodies.

Ibid. *Atque hinc, &c.* And thence also arose those vehement Convulsions, which the Author there mentions certain small Birds to have endured before their death.

p.89. *In mala, &c.* In a bad Cause they can do no other; but who compell'd them to undertake a bad Cause?

A Summary of the Contents of the several Chapters.

PART I.

W Herein the Adversaries Objections against the Elaterists are examined.

CHAP. I.

The occasion of this Writing, pag. 1. Franciscus Linus his civility in writing obliges the Author to the like, p. 2. Books concerning the Torricellian Experiment wherewith the Author was formerly unacquainted, ibid. The Inconvenience of Linus's Principles, ibid. The division of the ensuing Treatise into three parts.

CHAP. II.

A repetition of the Adversary's Opinion and Arguments. His Arguments against the Weight of the Air examined, p. 4. An Experiment of his to prove that the external Air cannot keep up twenty Inches of Quick-silver from descending in a Tube twenty Inches long, ibid. The Author's answer and reconciliation of the Experiment to his Hypothesis, p. 5. and the relation of an Experiment of the Author's, wherein only water being employed instead of Quick silver, without other alteration of the Adversaries Experiment, it agrees well with and confirms the Author's Hypothesis, and his Explication of the mentioned Experiments, ibid. That Water hath no Spring at all, or a very weak one, p. 6. The second Argument examined, ib. Whether the same quantity of Air can adequately fill a greater space, p. 7. The conceivableness of both Hypotheses compared, ibid.

CHAP. III.

Another Argument of the Adversaries, from an Experiment wherein the Mercury sinking draws the Finger into the Tube, examined. Q: Whether the Mercury placed in its own station is upheld by the external Air, or suspended there by an internal Cord? p. 7, 8.

CHAP. IV.

A repetition of Franciscus Linus his principal Experiment, wherein in a Tube of twenty Inches long the Finger on the top is supposed to be strongly drawn and suck'd into the Tube, p. 8. The Experiment explicated without the assistance of Suction, by the pressure of the external Air upon the outside of the Finger, thrust, not suck'd in, p. 9. Franciscus Linus his argumentation considered, p. 10.

The Examiners last Experiment considered, in which he argues against the Author's Hypothesis, because Mercury is not suck'd out of a Vessel through a Tube so easily as Water is, p. 11, 12. An Experiment of Monsieur Paschall shewing, that if the upper part of a Tube could be freed from the pressure of all internal Air, the Mercury would by the pressure of the outward Air be carried up into the Tube as well as Water, till it had attained a height great enough to make its weight equal to that of the Atmosphere, p. 13. Why in a more forcible respiration the Mercurial Cylinder is rais'd higher than in a more languid, p. 14. A Remark by the Author, That the contraction of the Adversaries supposed Funiculus is not felt upon the Lungs, p. 15.

CHAP. VI.

The examination of the Adversaries 4th Chapter, p. 15. That the Spring of the Air may have some advantage in point of force above the Weight of it, p. 16. That it is unintelligible how the same Air can adequately fill more space at one time than at another, p. 17.

PART II

Wherein the Adversaries Funicular Hypothesis is examined.

CHAP. I.

Wherein what is alledged to prove the Funiculus is considered; and some Difficulties are proposed against the Hypothesis.

The nature of this supposed Funiculus described, p. 18. That according to the Adversaries Opinion this Funiculus is produced by Nature only to binder a Vacuum, p. 19, 20. The Adversaries proofs that there is no Vacuum examined, p. 20, 21. That where no sensible part is un-enlightened, the place may not be full of light, p. 21. The same true in Odours, ibid. That there may be matter enough to transmit the impulse of Light, though betwixt the Particles of that matter there should be store of Vacuities intercepted, p. 22. That a solid Body hath no considerable sense of pressure from fluid bodies, p. 24. Of the causes of the Vibrations of Quick-silver in its descent, p. 24, 25.

CHAP. II.

Wherein divers other Difficulties are objected against the Funicular Hypothesis. As that in Liquors of divers weights and natures, as Water, Wine and Quick-silver, there should be just the same weight or strength to extend them into a Funiculus, p. 27. That whereas the Weight and Spring of the Air is infer'd from unquestioned Experiments, the account of that Hypothesis is strange and unsatisfactory. As that the Quick-silver doth not only touch the top of the Glass, but stick to it; That Nature wreaths a little rarefied

fixed Air into a strong rope even able to draw up Quick-silver, p.27,28. That Rarefaction is performed by a certain unknown force, or vis divulsiva, *ibid.* That thin Surfaces are test successively one after another, that these Surfaces are continued into strings, that may be stretch'd without being made more slender, &c. p.29. The illustration of the manner how his Funiculus is made, from the rarefaction of Wax or Tallow in a lighted Candle, is considered, p.30. and shew'd not to be apposite, *ibid.* Divers other difficulties and improbabilities manifested in the Funicular Hypothesis, p.31. Of the inward Spring necessary to the contraction of his Funiculus, p.31,32. An Argument from a Pendulum's moving freely in an exhausted Receiver, that the medium it moves in doth not consist of innumerable exceedingly-stretch'd-strings, p.35.

CHAP. III.

The Aristotelean Rarefaction proposed by the Adversary examined. What Rarefaction and Condensation is, p.34. Three ways of explicating how Rarefaction is made, p.34,35. Absurdities in resolving the Magdeburg Experiment by the Aristotelean way of Rarefaction, p.36. The inconveniences of the several Hypotheses compared, p.37. The difficulties in the Adversaries explaining Rarefaction by Bodies infinitely divisible, *ibid.* The difficulties of explaining it by supposing Bodies made up of parts indivisible, p.39, 40. The difficulties wherewith his Condensation is incumbered, as that it infringes Penetration of Dimensions, &c. p.41.

CHAP. IV.

A Consideration pertinent to the present Controversie, of what happens in trying the Torricellian and other Experiments at the top and feet of Hills. That the Funicular Hypothesis is but an Inversion of the Elastical, one supposing a Spring inwards, the other outwards; one performing its effects by Pulsion, the other by Traction, p.46. That these trials on the tops and feet of Hills determine the case for the Author's Hypothesis, p.47. The truth of the Observation of Monsieur Paschall confirmed, p.48. and the several trials that have been made of it related, *ibid.* A trial of the Author's from the Leads of the Abbey-Church at Westminster, p.50,51,52. That the subsidence of the Mercury at the top of a Hill proceeds from the lightness of the Atmospherical Cylinder there, p.53. The relation of an Experiment lately made at Hallifax Hill in confirmation of the former, p.54.

CHAP. V.

Two new Experiments touching the measure of the force of the Spring of the Air compress'd and dilated. That it is capable of doing far more than the necessity of the Author's Hypothesis requires, p.55. The first Experiment, of compressing Air by pouring Mercury into a crooked Tube, related, *ibid.* Wherein the same Air being brought to a degree of density twice as

great, obtains a Spring twice as strong as before, p.57. A Table of the Condensation of the Air according to this Experiment, p.58. Particular Circumstances observed in the making the Experiment, *ibid.* How far the Spring of the Air may be increased, p.60. Of the decrement of the force of dilated Air, p.61. A Table of the Rarefaction of the Air, p.62. Particular Circumstances in making the Experiment whence this Table was drawn, p.63, &c. That the free Air here below appears to be near as strongly compressed by the weight of the incumbent Air as it would be by the weight of a Mercurial Cylinder of 28 or 30 Inches, p.69.

PART III.

Wherein what is objected against Mr. Boyle's Explications of particular Experiments is answered.

The entrance into this Part of the Discourse, with an advertisement how far only it will be requisite to examine the Adversaries assertions and explications, the Hypothesis on both sides being before considered, p.67.

A defence of the first and second Experiments, concerning the intrusion of the Finger into the Orifice of the Valve of the evacuated Receiver, p.68.

A defence of the third Experiment, why the Sucker being drawn down there is no greater difficulty in the end than in the beginning of the depression, *ibid.*

Of the fourth Experiment, touching the swelling of a Bladder upon the exhaustion of the ambient Air, and proportionably to that exhaustion, p.70, 71. The Author's and the Funicular Hypothesis in the explication of this Phenomenon compared, *ibid.*

Of the fifth Experiment, *ibid.*

Of the eighth Experiment, about the breaking of a Glass-Receiver which was not globular upon the exhaustion of the inward Air, p.71. Whether it were more likely to be broken by the pressure of the Atmosphere without, or a contraction of a string of Air within, p.72.

Of the ninth Experiment, *ibid.* Whether the breaking of the Vial outwards in the exhausted Receiver, was caused by the pressure of the Atmosphere through the Tube which was open to the ambient Air, p.73.

Of the 17. Experiment, p.74, 75, 76. The Torricellian Experiment being made within the Receiver, whether the descent and ascent of the Mercury in the Tube, under and above its wonted station, be caused by the debilitated and strengthened Spring of the Air, *ibid.*

Of the 18. Experiment, p.77, 78. Whether the Authors or the Funicular Hypothesis assign the more probable cause why a Cylinder of Mercury did in Winter rise and fall in the Tube, sometimes as Water in a weather-glass

glass according to the laws of Heat and Cold, and sometimes contrary thereunto, ibid.

Of the 19. Experiment, p.79.

Of the 20. Experiment, p.79,80. Some mistakes in the Adversary of the Author's meaning about the Spring of the Water, and the places whence the bubbles arose, ibid. The Hypotheses compared, ibid.

Of the 31. Experiment, p.81,82,83. Of the cause why the Marbles fell not asunder in the exhausted Receiver, though a weight of four Ounces were hung at the lower stone, ibid. Whether the account of the Author or Adversary be more satisfactory, ibid.

Of the 32. and 33. Experiments, of the re-ascent of the Sucker and its carrying up a great weight with it upon the exhaustion of the Receiver, p.84. How the flesh and neighbouring blood of a Patient is thrust up into a Cupping-glass, ibid.

Of the 37. Experiment, and the cause of the appearance of light or whiteness therein, p. 85.

Of the 40. and 41. Experiments, concerning the cause of the sudden death of Animals in the exhausted Receiver, p.85,86.

Of the 42. and 43. Experiments, p.87.

The Conclusion, p.91,92.

FINIS.

that according to the laws of East and Gold, and sometimes contrary there-

Of the 12th Experiment 1772.

On the 12th Experiment 1772. Some water in the Air of the
the 12th Experiment 1772. On the 12th Experiment 1772. On the 12th Experiment 1772.

Of the 12th Experiment 1772. On the 12th Experiment 1772. On the 12th Experiment 1772.

Of the 12th Experiment 1772. On the 12th Experiment 1772. On the 12th Experiment 1772.

Of the 12th Experiment 1772. On the 12th Experiment 1772. On the 12th Experiment 1772.

Of the 12th Experiment 1772. On the 12th Experiment 1772. On the 12th Experiment 1772.

Of the 12th Experiment 1772. On the 12th Experiment 1772. On the 12th Experiment 1772.

